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AN HYPOTHESIS TO ACCOUNT FOR THE MOVEMENT IN THE CRUST OF THE EARTH.[1]

I WISH to propound an hypothesis to explain the vertical movements in the crust of the earth. As preliminary thereto I propose to set forth the most elementary phenomena of dynamic and structural geology in a summary manner so as to exhibit the nature of the facts requiring explanation.

The earth is composed of four bodies surrounded by the ether.

First, there is a central nucleus constituting the principal mass.

Second, there is a crust of structurally disposed rock surrounding the nucleus, the thickness of which is comparatively small.

Third, there is an aqueous body surrounding the rocky crust, through which the islands rise, the largest of which are called continents. On these islands there are many lakes and rivers which ramify into innumerable brooks, creeks and rills.

Fourth, there is an aërial mantle of air extending to a limit which is not well determined.

Fifth, these four bodies, one outside the other, in succession, are surrounded by the ether.

The earth is thus composed of encapsulated globes enclosing a nucleus and bathed in ether, to designate which certain defini-

[1] Read at the November meeting of the National Academy of Science, Boston, 1897.

tive terms are needed. I shall, therefore, speak of the nucleus, the rocky crust or crust, the aqueous envelope or envelope, and the aërial mantle or mantle, and shall call them all spheres. For the sake of clearer distinction, these spheres may be called (1) the centrosphere, (2) the lithosphere, (3) the hydrosphere, and (4) the atmosphere. It must be observed that the ether is common to all of the celestial bodies, and perhaps penetrates them as it does the earth.

The centrosphere is the chief mass and has a density of 5.6. By reason of this great specific gravity, which is about twice that of the rocky crust, it is often supposed to be metallic. Geologic facts in a vast system lead to the induction that the centrosphere does not exist in the solid state; if it is metallic the weight reduces it to a trans-solid condition. To this condition the form of the earth testifies, as it is an oblate spheroid assuming the figure of a fluid under the combined action of gravity and rotation. There are facts which have led physicists to conclude that it must have a rigidity said to be equal to that of steel. This rigidity may be explained as a function of its rotation, revolution, and molecular motion, when the physicist and geologist would be in substantial accord.

The theory of a metallic centrosphere seems adequately to account for the trans-solid state, as the metals are found to flow under pressure; but the molten material which from time to time is brought to the surface from the interior of the earth never reveals this metallic constitution. It may be that there is a zone of matter beneath the structural rock and overlying the metallic nucleus which is penetrated by heat, now here, now there, and only these molten rocks are extravasated; or it may be that the solid state is limited by heat in one direction and by pressure in the other in such manner that all rocks flow under great pressure as do the metals.

The stony crust has been revealed by direct penetration to a depth of more than six thousand feet, but it is indirectly revealed in many regions to a much greater depth, perhaps in extreme cases to fifty or sixty thousand feet.

The islands of dry land have all been beneath the sea at some time or other, and all show that they have been submerged more than once, some more frequently than others. During that portion of the history of the crust, which is the theater of geological investigation, these periods of submarine condition in one region always appear to be contemporaneous with periods of subaëreal conditions in some other region. Thus there seem to have been regions of dry land and regions of ocean bottom coexisting with a large predominance of oceanic area.

The aqueous envelope covers the rocky crust over about three-fourths of its surface, and has an average depth of about twelve thousand feet, though in extreme cases the bottom of the sea is more than five miles below its surface, while in some few cases mountains rise to more than five miles above the level of the sea. It is certain that we are now able to study rocks which were deposited at depths much greater than that of the mean depth of the ocean, and there are many cases where rocks found on the summits of high mountains are known to have been deposited at great depths beneath the sea. Great regions of country are at one time submarine, and at another subaërial. These oscillations of upheaval and subsidence are oft-repeated in geological history, and the swing of oscillation seems to have been in some regions tens or scores of thousands of feet where they reach the maximum, and to be only tens or scores of feet at the minimum, so that the surface of the earth, in so far as it has been studied geologically, is found to give evidence of oscillations of level varying in these quantities.

These variations are geographically heterogeneous, one region may have its oscillation on a small scale, another on a large scale, the minor oscillations forming distinct geographical series and the major oscillations forming distinct geographical series; that is, one region has been subject during geological time only to minor oscillations, and another during the same time to major oscillations.

We must now more fully consider the nature of these movements. Sometimes upheaval is by anticlinal flexure, where the rocks are lifted along a line of upheaval and caused to dip away on either side in gentle or abrupt slopes which are sometimes beautifully curved; but such an upheaval often seems to be accompanied by a subsidence on the flanks. Symmetrical anticlinal flexures are not very common, but often one side slopes gently while the other is abruptly deflected. This abrupt slope is especially subject to rupture, in which case faults are substituted for flexures. Thus a block which dips gently in one direction has its margin, on the side of a fault, displaced as an abrupt escarpment. Blocks formed in this manner often careen upon their edges, so that the strata may become vertically disposed or quite overturned where the lower formed strata are found on top. Between careened blocks and flexed blocks no line of demarcation can be drawn: the same block in different parts of its course may be bent or broken, and the flexed blocks themselves be quite overturned. The rocks which are upheaved or depressed by faulting and flexing, one or both, are always found to be ruptured in line of the faults or flexures, and also transversely to them. This rupture is often minute, so that the sheets of rock are faulted and jointed and thus found in blocks of varying dimensions, but all very minute as compared with the widely spread formations from which they are broken. Thus the whole system of rocks, of igneous and aqueous origin alike, are broken into blocks by faults and ruptures, and still further divided by planes of deposition, so that the structural crust is a system of fragments sometimes with an area of many yards, other times an area of fractions of inches. When we compare these blocks with the great area of the structural crust we find that it is but an accumulation of blocks that are to the formations what grains of sand are to the blocks. We must now realize that the structural crust nowhere has a continuous coherence; that faults, joints, and partings render it a vast body of minute and loosely accumulated fragments. All of this upheaval and subsidence with flexures, faults, joints, and partings seem to

have been brought into this condition by intermittent convulsions often exhibited in earthquakes.

Having contemplated the lithosphere as a body moving in upheaval and subsidence, and shown what is about the maximum and minimum of these oscillations and their paroxysmal character, we are prepared to consider the structure of this crust.

In all geological ages volcanic eruptions have occurred and rocky material from the depths has been brought to the surface. Such appearances of lava at the surface have been very common in human history, and they appear to have been just as common in all the geological ages revealed by science. Lavas vary in chemical and mineralogical constitution, but this variation is within narrow limits. All of the mineral substances known to mankind appear, but are intimately mixed as minute ingredients. Lavas, therefore, are intimate mixtures of many substances, the average of which falls within narrow limits. It would appear from our present knowledge that the primordial surface of the earth was cooled lava and that lava has been erupted from time to time through all of the great geological ages.

Upon these cooled surfaces a new crust of rocks from below and rocks from above appears to have been spread. Wind waves and tidal waves are forever beating the lands and undermining the cliffs and distributing the materials beneath the sea. Then atmospheric agencies disintegrate the rocks and the rains wash the sands into the streams which carry them into the lakes and into the sea. By many cognate processes the lands are worn down and the sea bottoms built up; the amount of detritus thus accumulated in zones about the meandering shores is great, so that in regions of maximum activity formations are accumulated thousands of feet in thickness.

The winds contribute to the material which falls into the sea; plant life also furnishes its quota; accumulations of vegetation are ultimately consolidated among the formations as beds of coal; and animal life adds to the marine formations, for corals, shells, and bones are all brought to be buried in the sand, and often extensive formations of calcareous matter are

thus produced. From these sources the sedimentary rocks are brought to be mingled with the eruptive rocks and intercalated among them, while in turn they are thrust between the sedimentary rocks.

Layers of rock of sedimentary origin appearing as strata are commingled with other masses of rock of volcanic origin which come from the interior. Sometimes the lava flows under or between the sedimentary strata. When great masses of lava are found in these conditions they are called lacolites. Thinner sheets are called intrusive rock. Beds poured over the surface are called coulées. The floods of lava come through fissures and fill them both below and above coulées, intrusions, and lacolites. Such fissure formations are called dikes; where the lava comes forth in volcanoes, the orifices are filled with molten rock which consolidates and are then called chimneys; great bodies of lava are ejected by some volcanoes as scoria and ashes, and often the ashes are minutely comminuted; the expulsion of such material is doubtless due to the production of gases and vapors, especially of steam, and the comminution is probably due to the explosive actions of particles of water expanded into steam. Great volcanic cones are often formed by the piles of scoria and ashes which are extravasated, and the ashes themselves when highly comminuted are drifted by the wind, sometimes far away from the locus of eruption. Beds of ashes and scoria formed in this manner are called tuff. So the bodies of rock formed by eruption are commingled with the bodies formed by sedimentation, and all are known as formations. Both the sedimentaries and the eruptives undergo a further change, which to a greater or less extent obscures their origin, for the original formations are metamorphosed, that is, recrystallized and lithified; so that the planes of sedimentation are partly or largely obscured and the beds of lacolites, intrusive sheets, coulées, dikes, chimneys, and tuffs have a new structure imposed upon them, and are then known as metamorphic rocks.

An attempt has been made to define formations; now they must be considered in a new light.

The land areas have always been subject to degradation by rains, rivers, and waves, and the materials washed from the land have been carried into the sea and there deposited; thus the continuance of dry land area is comparatively ephemeral. Not only are the lands degraded in this manner, but when they reach the level of the sea they continue to subside; when above the sea they are speedily unloaded, but when brought to the level of the sea or nearly so the islands, though having their loads discharged, continue to sink. The regions which have received the detritus of the islands and are thus loaded by them, are elevated into the island or continental condition; thus land areas rise to be unloaded and then sink, while oceanic areas are loaded and then rise to become land areas. The extent of this upheaval and subsidence and the vertical movements, involved together with the vast transportation of material from land to sea, seems to be enormous when we contemplate the almost silent and unseen agencies by which it is accomplished.

In considering large areas of the surface of the earth, as, for example, the great continents or zones of archipelagoes, we reach certain generalizations of prime significance.

Regions of great denudation are also regions of great deposition, regions of great eruption, regions of great upheaval and subsidence, and also regions of great flexure and fracture; thus denudation and deposition, eruption, displacement, as subsidence and upheaval and as fracture and flexure, are correlated in this manner: that where there is more of one there is more of all; where there is less of one there is less of all.

Geologists have found no law, condition, or cause by which to explain these phenomena of the earth's crust as the law of gravity explains the constitution of celestial systems. The search for this law has been almost exclusively in one direction, under the hypothesis of a cooling and contracting earth, but with the lapse of time it has been found inadequate. Attempts have been made to compute the amount of contraction supposed to result from the wrinkling of the crust of the earth in anticlines and synclines. It seems to entirely fail quantitatively.

Contraction does not seem to be an explanation of all or even the chief phenomena which we have briefly set forth. When this hypothesis was considered, flexion seemed to be the chief method of displacement; now we know that fracturing and faulting is the chief method in regions of maximum action. When inclined rocks are studied they seem to have been stretched, as evidenced in the elongation of particles transverse to the strike, and they seem further to have been stretched by the opening of fissures and joints. Altogether it may be affirmed that displacement does not teach the doctrine of a contracting earth, or, if that statement is too strong, it does not give evidence of a sufficient contraction necessary to the hypothesis, and it also fails to explain the concomitant phenomena.

With this hypothesis another is associated, namely, that the centrosphere of the earth is metallic, for which no vestige of inductive evidence has yet appeared; and the stupendous fact remains that the centrosphere has more than twice the density of the crust. All eruptive rocks which come into the purview of science are found to have an average constitution which is about the same as that of the sedimentary rocks. It is found by experiment in the industrial arts that under pressure metallic and other substances flow; and geology teaches that all of the other rocks are secularly deformed under differential pressures, so that rocks highly metamorphosed in this manner are twisted, contorted, and kneaded into new shapes. Finally, there is now abundant geologic evidence to show that the faulting near the surface appears as flexure at greater depths, and finally that flexure appears as molecular readjustment at still greater depths, expressed in slaty structure where the particles of the rocks are rearranged in parallel planes.

The metals of the normal condition have great density, but in a pure condition are found only in exceedingly minute quantities; all the other rocks have a small density. If now we assume that all rocks flow under pressure, that the critical point is variable and that the modulus of compression is also variable, being greater for the lighter rocks and less for the heavier, and that

this modulus is greatly accelerated at the critical point, we have a law which will regiment the facts of geonomy as the facts of astronomy are marshaled by the law of gravity.

Under this theoretic law of the accelerated modulus of compression at the critical point for different substances, subsidence and upheaval are explained. The reassumption of constitutional structure in crystallization and glassy lithification necessitates expansion, and thus upheaval is explained. When lands rise and are denuded, the process of relithification in the centrosphere continues upheaval and exposes the lands to further upheaval, and this process goes on until an equilibrium is reached at the epoch when the land is brought to the level of the sea by degradation. On the other hand, as land is loaded the subjacent crust rocks are brought within the zone of accelerated compression, and this process continues while the loading continues until it is brought to a close at the epoch when the land area from which the detritus is taken is brought to the level of the sea and transportation ended so that loading ceases.

Universal contraction by cooling must still be postulated as an agency for the destruction of equilibrium, or perhaps we may find this agency in astronomical conditions; but some such agency is necessary for the continuation of the process. But the changing of material from the interior to the surface and the changing of load from one district to another by transportation under the law of the accelerated modulus of compression is the principal agency of upheaval and subsidence.

This doctrine was proposed several years ago by myself, but has received little attention except among a few geologists engaged in this branch of research; from its reception by these gentlemen I am encouraged to repropound it.

J. W. POWELL.

ESTIMATES AND CAUSES OF CRUSTAL SHORTENING.[1]

INTRODUCTORY.

IN THE following paper I shall use the words crust and nucleus as terms by which to conveniently refer to the outer known solid shell of the earth, of which we have direct knowledge, and to the core surrounded by the crust, of which we have only inferred knowledge. The use of these terms in this sense is independent of any hypothesis as to a sharp boundary between the two, and of any theory as to the condition of the interior of the earth. So far as my present purposes are concerned, the nucleus may be entirely liquid, entirely solid, part liquid and part solid, or in a state of matter of which we have no knowledge.

The intricate phenomena of earth deformation, and particularly that form of deformation called folding, has led geologists to assume, in order to account for the facts in the field, that the surface of the earth must have been vastly shortened during geological time. Some instances of the estimates of the amount of crustal shortening may be mentioned.

Dutton[2] thinks, to explain the phenomena of folding since the close of the Cretaceous, that the radius of the earth must have been shortened more than thirty miles. He states that the plications are so great that we must assume a contraction on some circles of latitude since the commencement of the Permian amounting to many hundreds of miles, and this amount of contraction is small, he says, compared with that involved in the Laurentian rocks. Heim[3] estimates the transverse shortening of

[1] Published by permission of the Director of the U. S. Geological Survey.

[2] A criticism upon the contractional hypothesis, by C. E. DUTTON: Am. Journ. Sci., Vol. VIII, 1874, p. 121.

[3] Mechanismus der Gebirgsbildung, von ALBERT HEIM: Basel, Band II, 1878, S. 213.

the Alps to be seventy-four miles. Le Conte[1] estimates the transverse shortening of the Coast Ranges of California to be from nine to twelve miles. Claypole[2] estimates the transverse shortening of the Appalachians in Pennsylvania to be forty-six miles. McConnell[3] thinks the folding of the Laramie range of British America required a transverse shortening of twenty-five miles.

In the above estimates of shortening by Heim, Claypole, Le Conte, and McConnell, I have inserted the word *transverse*, to call attention to the fact that shortening in only one direction has been considered by these authors. It is clear that to obtain an adequate idea of the effect of crustal corrugations, it is necessary to know in square miles the surficial lessening of the crust of the earth as a result of deformation. However, it appears that if in other mountain ranges the shortening is proportional to the estimates above given, the total amount of surficial decrease must be enormous. This would be true even if the deformation of ancient mountain ranges, the stumps of which are buried under later rocks, were ignored. Moreover, it is possible that the amount of folding and consequent shortening of the Paleozoic and older rocks, buried under the Mesozoic and Cenozoic strata, may be as great or even greater than the amount of shortening involved in the deformation observable at the surface.

The theory of mountain-making as a result of secular cooling has been repeatedly attacked along the lines of the vast contraction demanded by the supposed facts of the field, and the small contraction resulting from secular cooling, the only cause ordinarily assigned for contraction. Dutton[4] calculates that the

[1] On the structure and origin of mountains, with special reference to recent objections to the contractional theory, by JOSEPH LE CONTE: Am. Journ. Sci., Vol. XVI, 1878, p. 98.

[2] Pennsylvania before and after the elevation of the Appalachian mountains, by E. W. CLAYPOLE: Brit. Assoc. Rept., Montreal meeting, 1884, p. 718.

[3] Geological features of a portion of the Rocky mountains, by R. G. MCCONNELL: Geol. Surv. of Can., Ann. Rept., Vol. II, Pt. D, 1886, p. 31.

[4] Loc. cit., p. 120.

contraction due to secular cooling is mainly confined to the outer 200 or 300 miles of the earth, and states: "Although no estimate can be made of the contraction of this portion, it is probably safe to say that its volume cannot have been diminished so much as one-tenth; and if we were to assign thirty miles as the diminution of the earth's radius since the formation of a cooled exterior, we should probably reach the utmost limits consistent with Fourier's theorem."

It is believed, upon the one hand, that there may have been great overestimates of the amount of crustal shortening, and upon the other hand, that important causes for nucleal contraction may exist which have not been sufficiently considered. It is the purpose of this paper (1) to examine the evidence upon which estimates of crustal shortening have been made, and to consider the methods to be followed in making estimates of shortening, and (2) to summarize the known causes which may exist for nucleal contraction and crustal corrugation. The paper may thus be divided into two parts. In Part I, I shall consider the shortening of the outer surface of the earth accompanying folding, faulting, jointing, cleavage, fissility, and vulcanism and cementation; and in Part II, I shall consider the causes which may account for the shortening represented by the phenomena.

PART I. ESTIMATES OF CRUSTAL SHORTENING.

Folding.—The deformation of folding undoubtedly involves shortening, but it is believed that it does not necessarily require nearly so much shortening as has been believed. Estimates of shortening resulting from folding have not considered the effects of the following phenomena: (1) the thinning of the layers produced by folding; (2) the composite character of folds and the rapid variations in the closeness of the folds of the various orders; and (3) the effect of gliding on the limbs of folds.

1. In order to make an estimate of the amount of shortening involved in folding, it is necessary to recall the nature of the deformation of the individual beds and formations. It has been

shown[1] in another place that the mashing, flowage, and the shearing motion involved in differential movement between the layers necessarily involves thinning of the limbs of the folds or thickening of the troughs and crests, or both. Even where the

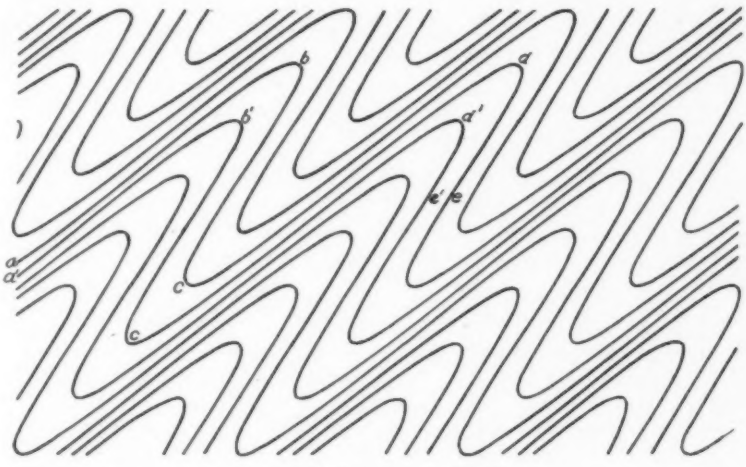


FIG. 1.— Similar monoclinal folds.

folds are not close, in case the folds are similar,[2] the limbs may not be much more than half as thick as are the troughs and crests (Fig. 4). This distortion becomes more and more important as the folding becomes closer, and in isoclinal and monoclinal folds, in which the strata turn back upon themselves, the amount of thinning of the limbs or thickening of the troughs and crests is very great (Fig. 1). A layer when traced out in such a set of folds alternately thins and thickens, and the section if developed on a plane, would alternately greatly widen and narrow (Fig. 2). The length of the developed layer should be the length of its central part. The excess of material for each

[1] Principles of North American pre-Cambrian geology, by C. R. VAN HISE: 16th Ann. Rept. U. S. Geol. Survey, Pt. I, 1896, p. 599.

[2] Loc. cit., pp. 599–600.

curve on the convex side of such a central line would equal the deficiency on the concave side, and consequently such a developed layer truly represents its mass and average length. If it be assumed that the original horizontal stratum had a

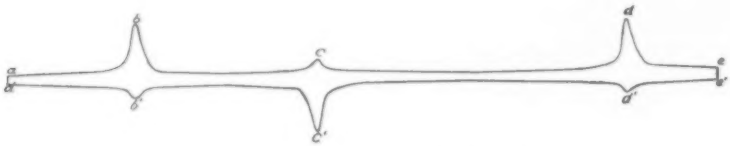


FIG. 2.— Development of a part of a layer of Fig. 1.

thickness equal to the thicker parts of the folded stratum, it would follow that the developed stratum is much longer than it was originally. However, probably in no case is this assumption wholly correct, and in many cases it is far from the truth. The weaker and stronger layers require consideration separately.

At most places the evidence seems to show that during deformation the anticlines and synclines of the weaker layers have been thickened. However, since during the deformation there is shearing motion under pressure all along the limbs, it can hardly be doubted that in many cases the thinning of the layers is a more important phenomenon so far as their length is concerned, than thickening on the anticlines and synclines.

In the stronger layers, often no evidence of thickening is anywhere seen. Upon the contrary in many cases, as will be shown below, the layers are actually elongated by tensile fracture upon their convex sides and therefore cannot have been thickened. Radial open fractures upon anticlines and synclines, due to tension, are beautifully exhibited in the southern Appalachians (Fig. 8). The openings in many cases have been filled with quartz. These joints are evidences of tensile forces. Where stretching of anticlines and synclines occurs in the zone of flowage, this is undoubtedly due to the great friction between the layers, and to their positions on the convex sides of the neutral planes. The limbs of the stronger layers, like those of the

weaker beds, are subjected to differential shearing under pressure, and if distortion occurs they must be thereby thinned.

The actual thinning and elongation of layers as a result of folding has been noted by Le Conte[1] as an important phenomenon in the Coast Ranges of California, and by Reade[2] in various mountain ranges.

According to Gilbert,[3] during the introduction of igneous rocks, which formed the Henry mountains, the pressure of the magma normal to the strata was so great that they were extended laterally by flowage a sufficient amount to cover the domes. In the case of the Holme's arch the linear extension was about 2 per cent.

The amount of thickening or thinning, which any given formation or layer undergoes, will of course depend upon many factors, among which attitude, strength, pressure, amount of differential movement or shearing are to be considered.

As noted by Reade,[4] the attitude of the layers is of the greatest importance. In their initial position the tendency of the pressure is to thicken them. This tendency continues as the layers are tilted, until the average dip is 45°. As soon as the layers upon the average have a greater inclination than 45° (Fig. 1), the average effect of the tangential pressure is unquestionably to thin the layers, although some members at certain places, and especially at the turns, may be thickened. When it is remembered that in the closely-folded mountains the layers generally have dips greater than 45°, and as explained later (pp. 16–17) such layers usually turn quickly at the anticlines and synclines, it becomes evident that the thinning of the layers and their consequent elongation, as a result of tangential pres-

[1] On the structure and origin of mountains, with special reference to recent objections to the contractional theory, by JOSEPH LE CONTE: Am. Journ. Sci., Vol. XVI, 1878, pp. 299, 301, 302.

[2] The origin of mountain ranges, by T. MELLARD READE: London, 1886, pp. 176, 268, 211.

[3] Geology of the Henry mountains, by G. K. GILBERT: Rept. U. S. Geog. and Geol. Surv. of the Rocky mountain region, 1877, pp. 80–82.

[4] Loc. cit., pp. 216–217.

sure in positions which average greater than 45°, may be very important. In reference to the other factors, the greater the pressure, the greater the tendency for thickening and shortening at angles less than 45°, and the greater the tendency for thinning

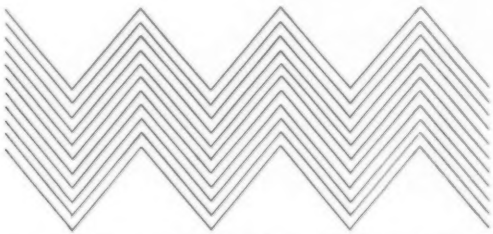


FIG. 3.—Similar upright folds with angular crests and troughs.

and lengthening at angles greater than 45°. The greater the shearing between the layers, the greater the thinning. The greater the rigidity of any given layer, the less the thinning.

The foregoing statements are understood to apply to strata which are so deeply buried that the deformation of the layers results from true interior distortion of them. In the upper zone of fracture these statements need to be modified, as subsequently explained.

With present data only these qualitative statements can be made. But, it seems probable that in the earlier stages of the development of folds, the average thrust thickens instead of thins the layers. However, where the folds are very close, and especially in isoclinal and overturned folds, it can hardly be doubted that upon the average there is considerable thinning and consequent important elongation of the layers. For folds of a given average closeness the average amount of distortion is not so great where the strata bend back upon themselves with sudden turns, as where the bends occur gradually (compare Figs. 3 and 4), although the distortion at the angles may be greater than at the corresponding places upon the gentle turns. In nature the deformation is ordinarily between the two extremes figured.

The fact that the distortion is less in folds with sudden turns than in those with rounded turns, is believed to be a cause why this deformation so frequently occurs in closely-folded rocks. The angular deformation requires less work, and there-

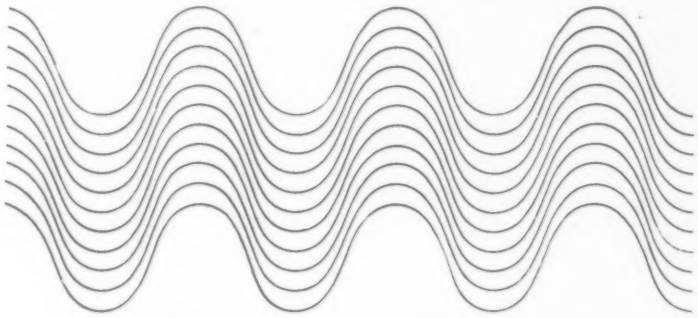


FIG. 4.—Similar upright folds with rounded crests and troughs.

fore less energy than the other form. As the stresses slowly accumulate until the elastic limits of the rocks are surpassed, the deformation which will occur in any given case in order to relieve the stresses is that which requires the least expenditure of energy under the existing conditions. This is the old principle that a mass under stress gives way along the lines of least resistance.

2. In estimating the amount of superficial shortening involved in folding, it is necessary to consider what and where strata shall be selected for estimation. I have shown that if there is no thinning or thickening of the layers (Fig. 5) folds rapidly die out above and below any given folded layer. I have also shown that similar folds are only possible by profound distortion of the layers (Figs. 1 and 4), unless the turns are very abrupt. Agreeing with theory, actual geological sections are a compromise between parallel folds and similar folds, the folds rapidly varying in closeness in different parts of a mountain mass, vertically and laterally.[1]

[1] Principles of North American pre-Cambrian geology, by C. R. VAN HISE: 16th Ann. Rept. U. S. Geol. Surv., Pt. I, 1896, pp. 599–601.

In this connection it should be remembered, if the theory of a level of no lateral stress be true, that a good reason exists for the lessening folding and distortion of layers with increasing depth.

Whether or not this theory as ordinarily stated approximates

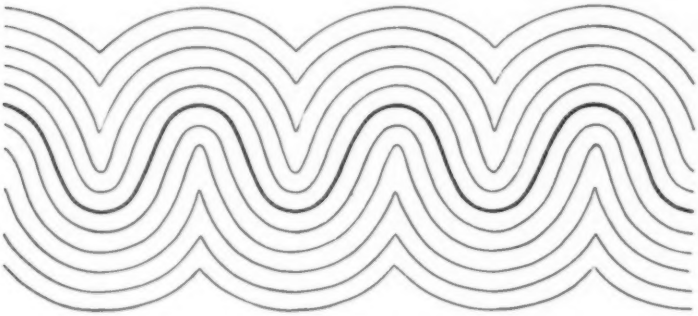


FIG. 5.— Parallel upright folds with rounded crests and troughs.

quantitative correctness, it is certain that the amount of shortening must somewhere decrease with increase of depth; for infinitesimally near the center of the earth the amount of shortening must be infinitesimally small. Since with present knowledge we can only conjecture the law under which folds die out in depth, though we are certain that they must die out, one is not justified in assuming that folds similar to those at the surface continue even for moderate depths.

If this principle be ignored in estimating shortening, a serious error may be made. The formation being followed may plunge beneath softer formations which show close plications. If it be assumed that similar plications also effect the formation below to be measured, this may lead to a considerable overestimate of the amount of crustal shortening (Fig. 6).

Also the lateral variation in closeness of folding may lead to error. If the layer or formation to be measured is not continuously exposed, it may be visible where it chances to be most closely folded and be concealed where more openly folded. If at

the places of less folding the strata chance to be hidden, the plications of other strata must be selected to fill in the gap. Layers may be selected which exhibit close folding. But even if layers are selected which show the least folding of any in sight,

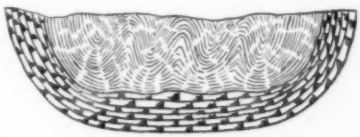


FIG. 6.—Simple fold below composite folds.

there is still a possibility that a considerable overestimate may be made of the closeness of folding and of the amount of crustal shortening.

For oftentimes, where a formation upon which estimates of shortening are being made, disappears below the surface, this results from its plunging downward as a part of a synclinorium. It is believed that upon the average synclinoria are less closely folded than anticlinoria. Anticlinoria are places of crustal thickening resultant upon close plications, whereas synclinoria are areas ot depression as compared with the anticlinoria, but not really areas of depressior as compared with the unfolded districts. If the plications of synclinoria were as composite as anticlinoria, this would involve an equivalent amount of thickening of the crust, and consequently equal elevation with the anticlinoria unless a large amount of material, to compensate for the difference in elevation, had flowed from below the synclinornia to below the anticlinoria. Doubtless the flowage of the kind suggested does take place to some extent, but to no such extent as would be involved in this hypothesis

Willis's experiments most beautifully illustrate the composite character of the folding of anticlinoria as compared with the intervening synclinoria.[1] The above reasoning applies exactly

[1] The mechanics of Appalachian structure, by BAILEY WILLIS: 13th Ann. Rept., U. S. Geol. Survey, Pt. II, 1893, Pls. LXXVII, LXXXI, LXXXII, LXXXIV, LXXXV, LXXXVI.

to his experiments. When the strata were compressed in these experiments there was flowage from below the synclinoria, but not a sufficient amount to allow the synclinoria to become as plicated as the anticlinoria, and at the same time to be at a lower level.

3. In anticlinal mountain masses the cores, composed of the oldest and originally deepest-buried strata, are often less closely plicated than the strata on the flanks of the mountains, which are composed of younger rocks. In estimating the crustal shortening of such mountain masses, we have, therefore, the

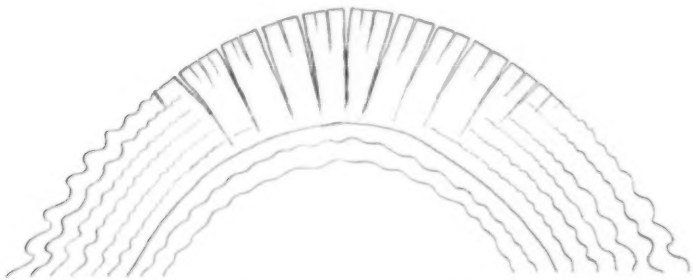


FIG. 7.— Possible relation of secondary folds to joints.

difficulty already mentioned, that is, folds of different layers vary in their closeness. This occurs notwithstanding the fact that upon the limbs one would expect that differential movement between the layers, or shearing, which tends to stretch them rather than to produce plications, are at a maximum. The greater folding of the limbs, as compared with the cores may be partly explained by the principle already given, that in general folds die out as depth increases, and consequently the older strata are least folded. However, the plications are probably to be chiefly explained by the gliding of the material down the slope upon the flanks of the mountain, because under the stress of gravity (Fig. 7). The strata on the crests may have been removed by erosion, or, as explained in another place (see p. 24), the stresses may have there produced joints. In either

case the material on the flanks is no longer held in position by the tensile strength of the rocks on the crest, and under the stress of gravity slides down the slope, and this results in corrugations. The plications upon the flanks may thus be partly or fully compensated by separation of the material along the crests. However, the plications may be inferred, in estimating crustal shortening, to have extended to the part removed by erosion. In this case the amount of crustal shortening due to folding would be overestimated. In reality, the original length of the strata was that of a gentle continuous curve of the order of magnitude of the mountain mass.

The question may be asked as to the reality of the existence of the gliding effect above assumed as a result of the action of gravity. In another place[1] I have fully discussed the forms of the secondary folds which occur in composite anticlinoria and synclinoria of the first order. It there appears that the secondary folds upon the flanks of the mountains so commonly have attitudes which must have resulted from this gliding effect, that the composite folds, the secondary folds of which show such attitudes, have been called normal composite folds. This discussion cannot be here repeated, but if the argument given be correct, the gliding effect due to gravity producing secondary corrugations upon mountain flanks is a significant phenomenon, and consequently the cause here assigned for overestimates of the amount of crustal shortening is of importance.

It is clear that in appealing to the force of gravity to pro duce corrugations upon the slopes of the mountains, I am follow ing Dana[2] and Reyer.[3] However, I do not follow the latter fully. He makes the gliding the cause of the formation of mountains, whereas, it is clearly an effect, following the mountain-making. Material cannot glide down until it has been raised up. My

[1] Principles of North American pre-Cambrian geology, by C. R. VAN HISE: 16th Ann. Rept. U. S. Geol. Surv., Pt. I, 1896, pp. 608–615.

[2] Geological results of the earth's contraction in consequence of cooling, by JAMES D. DANA: Am. Journ. Sci., Vol. III, 1847, p. 185.

[3] Theoretische Geologie, by E. REYER: Stuttgart, 1888, p. 829.

present point is that if the corrugated parts are alone considered, and the jointed part or the part removed by erosion ignored, that the average crustal shortening may be greatly overestimated.

If the considerations presented in the foregoing pages have weight, it is clear that the actual measurements in the field of the amount of crustal shortening involved in folding presents great difficulties, and the question naturally arises as to the best practicable methods of procedure.

1. So far as practicable, the same formation should be measured throughout a section, and if it is necessary to transfer from one formation to another, the greatest care should be exercised in order to avoid the errors which may result from changing from a formation to another lower or higher, and also to avoid the error which may come in as a result of the lateral change in closeness of plication.

2. The strongest formations available should be selected for measurement.

This selection should be made because the stronger formations have less composite curves than the weak ones. As a consequence they are less distorted during the folding than the weak formations. These facts may be observed in almost any good section of closely-folded heterogeneous strata. The more composite crenulation, but not the greater thinning and thickening, of the weaker layers may be illustrated by bending a rectangular pile composed of bunches of paper alternating with cardboards, the pile being held firmly either mechanically or with the fingers at the edges, so that slipping between the laminæ may be hindered at the places held. In this experiment, at the crest or trough, spaces form between the stronger layers, and in these spaces the weaker layers take on secondary crenulations. In natural geological sections the pressure upon the limbs is frequently sufficiently great so that the material of the weaker layers flows toward the openings on the anticlines or synclines, and partly or wholly occupies them. In many places some of the weaker layers are quite pinched out upon the limbs.

The physical cause for the simple folding of strong layers

and the composite folding of the weaker layers is that already assigned for another kind of deformation on p. 17, namely, under given conditions, the deformation occurs which demands the least expenditure of energy. To deform the strong layers in a composite way requires a large amount of energy. To deform the weak layers in a composite way requires much less energy. The simple deformation of the strong layers and the composite deformation of the weak layers demands less energy than would be required to deform all the layers in a similar manner, so that the deformation would average the same as in the case of the unequal deformation of the strong and weak layers.

Under different circumstances the strong layers vary greatly in the simplicity of their deformation. In case the load is not too great, as explained by Willis, the strong layers are bent into large, simple folds. If, upon the other hand, the load is too great for the strata to support, the strong layers are folded in a composite manner. Both of these cases fall under the principle that the deformation occurs which requires a minimum expenditure of energy, for in the case of the lighter load, it requires less energy to elevate the load on the anticline, or to somewhat depress it on the syncline, than it does to greatly distort the strong formations. But in the case of the great load it requires less expenditure of energy to distort the strong layer a sufficient amount to make it develop composite folds than it does to elevate the load. But as above stated, even in the case of composite folding of the stronger layers, the weaker layers adjacent to them show still more composite deformation.

The statement made that the strong formations should be selected for tracing above the surface and for measurement is therefore justified.

3. From the places where the strong formations plunge below the surface to the places where they reappear, only the most gentle curves should be assumed (Fig. 6).

4. From the places where the formation which is being measured is lost because removed by erosion, only the most

gentle practicable curves should be assumed to the places where the formation reappears, and even if this be done, as shown (pp. 20–22) an overestimate may be made of the length of the formation.

5. It should be ascertained whether the formation measured has upon the average been thinned or thickened, and a corresponding allowance should be made.

If the principles are not appreciated upon which the foregoing precautions are based, with the natural, indeed almost inevitable tendency for one to pick out strata for measurement which have suffered severest deformation here, and severest deformation there, we may be sure that estimates of shortening will have comparative little value.

Jointing.—In another place[1] I have explained that joints may be of two kinds, tension joints and compression joints. Tension joints in simple folds may form in one direction at right angles to the bedding, or nearly so, in the zone of fracture (Fig. 8). In the case of complex folding, two sets of tensile joints intersecting each other at right angles may develop, both, however, still normal to the bedding or nearly so. Compression joints, forming in shearing planes, are ordinarily more or less diagonal to bedding. However, the greatest compressive stresses may approximate angles of 45° to the bedding, in which case the shearing fractures would be nearly normal to bedding. Compression joints, like tension joints, may develop in two directions at right angles to each other.

In the gentle folds of the Paleozoic of the Mississippi valley and the strata of the plateau country of the far West, joints are normal to the bedding, or nearly so, corresponding in position to the direction of the folding. For instance, southern Wisconsin is a gentle southward-plunging anticline, in other words, the principal fold has a nearly north-south axis, and the rocks dip east to Lake Michigan and west to the Mississippi river. Corresponding to this arrangement are numerous joints in a north-south direc-

[1] Principles of North American pre-Cambrian geology, by C. R. VAN HISE: 16th Ann. Rept. U. S. Geol. Survey, Pt. I, 1896, pp. 668–672.

tion. However, the plunge of this anticline varies in passing from north to south. In other words, there is here a great but very gentle cross fold, and corresponding to this is another set of joints which run in an east-west direction.

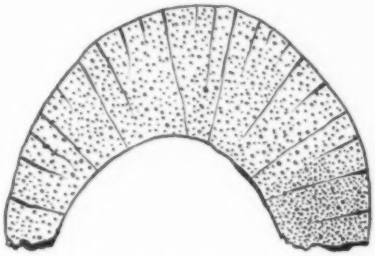


FIG. 8.—Tensile joints.

The same arrangement of joints is still better illustrated in the closely folded Allegheny mountains, and the Coast Ranges of Oregon and California. In the Allegheny mountains, as may be seen by sections along the railroads (for instance, the Pennsylvania, and Baltimore and Ohio) in the stronger beds there are two sets of joints everywhere corresponding to the strike and dip, in other words, corresponding to the two directions of fracture due to longitudinal and transverse folding.

Such joints may be seen both in anticlines and synclines. They occur in sandstone, grit, or limestone. Where the layers are a foot or more in thickness, and the rocks are gently folded, the joints may be several feet apart. Where the layers are closely folded the joints are frequently less than a foot apart. In the thinner layers, those from two to six inches in thickness, the joints are ordinarily less than a foot apart, and where closely folded are but two or three inches apart. Indeed, in some cases of close folding, the two sets of joints are so close together as to break the formations into a set of parallelopiped blocks, the dimension along the bedding being the least of the three, that is, the joints are closer together than the thickness of the beds.

It is apparent that the Paleozoic strata of the Mississippi valley and of the Plateau region, the Alleghenies, and Coast Ranges were folded under such conditions that the curves of the folds were produced not by actual bending of the layers, but

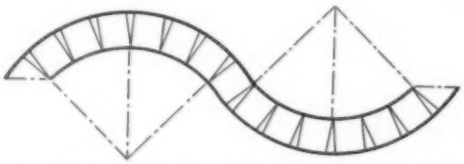


FIG. 9.— Diagram of radial openings produced by tensile fracture.

by numerous fractures, with a slight displacement of each block, resulting in a curved form (Figs. 9 and 10).

Now these joints must have been produced by tensile stresses or by shearing stresses. If they are of the first class, it is self-evident that the production of the joints involved surficial elongation (Fig. 9). If they are of the second class, their production may have involved all of the surficial elongation (Fig. 10), and it will be explained in a subsequent number of this JOURNAL[1] that joints of this kind are believed to be widespread. Some reasons for this belief may here be mentioned. These joints in many regions show a marked tendency to a vertical attitude, as in figure 10. Also the kind of displacements generalized in figure 10 has been observed at various places. Moreover, such joints are closer together the closer the folding, and in some cases they are so close as to make the intervening masses approach leaflets, as, for instance, in sandstones and shales on the Chesapeake and Ohio canal, three miles west of Hancock, Md.

In both the cases of joints produced by tension and shearing above described, there is no real elongation of the strata, but merely a displacement of the blocks causing surficial elongation. In the case of the tension joints this elongation is due to the fact that spaces are measured; in the case of the

[1] Deformation of rocks, by C. R. VAN HISE: JOURN. GEOL., Vol. VI, 1898.

shearing joints the apparent elongation is due to the fact that the measurements are diagonally across the blocks, instead of following the bedding.

From figure 9 it is plain that the average theoretical elongating effect of tension joints is directly as the thickness of the layers or formations through which the joints continuously extend, and indirectly as the radius of curvature. In the field it oftens happens that as a result of the position of a layer or formation upon the convex side of the neutral plane of deformation, the different blocks are separated from one another on the concave sides of the curves as well as on the convex sides.

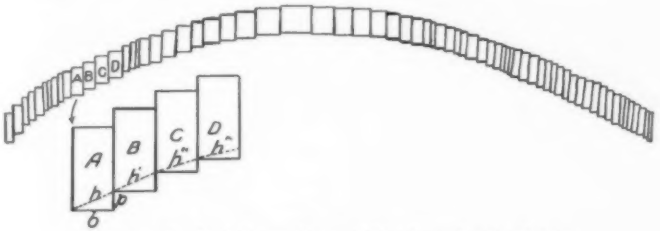


FIG. 10.—Surficial elongation resulting from shearing joints.

From figure 10 it is plain that the surficial elongating effect of the shearing joints is great in proportion to the displacements along the joints, and to their frequency. The apparent length in any case is the sum of the hypotenuses of the right angle triangles ($h+h'+h''+h'''$, etc., Fig. 10), the bases of which are the lengths of the blocks parallel to the bedding, and the perpendiculars of which are the displacements.

It is not to be concluded from these illustrations that there is no crustal shortening as a result of joint folding. Shortening might occur even if the entire bending were accomplished by tensile joint fracturing. Also in the case of the shearing joint fracturing the rubbing of the blocks against one another might produce shortening.

However, if an estimate of the original surface of the layers were made, upon the supposition that it was as great as it would appear to be if developed on a plane, this would result in a

considerable overestimate of the amount of its original surface.

In many of the weaker layers of folded rocks is a diagonal jointing, due to differential motion, and cutting this diagonal jointing nearly at right angles is a diagonal fissility.[1] The whole may result in thinning the limbs of the folds, just as does the shearing motion in the case of folding by plastic flow.

It is very desirable that the quantitative value of the lengthening effect of jointing should be known for various kinds of deformation. This, however, is an exceedingly difficult task. The quantitative value of the surficial elongation due to jointing for any deformation of an area can be only approximately determined after an extensive and close field study of the district. Consequently, for the present, I am obliged to be content with comparative statements which rest upon my own judgment, and which may be questioned by other observers. I believe the elongating effect of jointing to be quantitatively of sufficient importance that it should be taken into account in estimates of crustal shortening. I believe the lengthening effects of joints are important in connection with the estimates of shortening due to folding where the folds on the flanks of mountains may be due to a downward gliding effect, and be compensated by the joints (see Fig. 7), as explained on pages 20–22. However, I suppose that the elongating effect of jointing is not so great as that of the distortion of closely folded rocks in the zone of flow, as explained on page 16.

Faults.— Faults are ordinarily classified into normal faults and reverse faults. The normal faults involve an elongation of the crust of the earth as certainly as the reverse faults involve a shortening of the crust of the earth.[3] The very names, normal and reverse faults, show that the first are of far greater abundance,—are in fact the rule.

[1] Principles of North American pre-Cambrian geology, by C. R. VAN HISE: 16th Ann. Rept. U. S. Geol. Survey, Pt. I, 1896, pp. 651–654.

[2] Supplementary notes on deformation of rocks, by C. R. VAN HISE: JOURN. GEOL., Vol. V, 1897, pp. 190–191.

[3] Principles of North American pre-Cambrian geology, by C. R. VAN HISE: 16th Ann. Rept. U. S. Geol. Survey, Pt. I, 1896, p. 674.

However, since the hade of a reverse fault is usually flatter than that of a normal fault, the shortening due to a given vertical displacement of a reverse fault exceeds the shortening due to the same displacement of a normal fault. In considering the elongation or shortening of the crust of the earth by faults, this factor must be considered, as well as the factor of their relative frequency and area of distribution. However, reverse faults are usually confined to closely-folded areas, while normal faults frequently occur in these same areas, developing in the final stages of deformation, and are also present throughout great regions where reverse faults are absent, as, for instance, in the Great Basin and Plateau regions. It is therefore wholly possible that the amount of shortening of the crust of the earth resulting from reverse faults is more than compensated by elongation of normal faults, and thus the sum total of deformation by faulting result in dilatation rather than shortening of the crust of the earth.

Cleavage.—Cleavage has been supposed necessarily to indicate an important shortening of the crust of the earth.

It is, however, to be remarked that the shortening of cleavage must not be considered if the amount of shortening involved in the folding is counted for the same region; for cleavage is a phenomenon which may result from distortion under conditions of flowage, and the shortening represented by folding includes that involved in the simultaneous production of cleavage.

Moreover, cleavage is possible without any shortening whatever. I have shown in a previous paper[1] that cleavage may be produced by simple shearing motion parallel to the surface of the earth. The inclination of the cleavage will depend upon the amount of the shearing. Shearing of a very moderate amount will produce cleavage with dip as low as 30°. In the production of cleavage by shearing, each individual particle is shortened. Where shearing motion parallel to the surface of the earth results in cleavage inclined at 30°, the amount of shortening of each particle is about .4. However, the direction of shorten-

[1] Deformation of rocks, by C. R. Van Hise: Journ. Geol., Vol. IV, 1896 pp. 636–637, 868–872.

ing is inclined to the surface of the earth. The shortening involves an equivalent elongation in another direction. This elongation is at right angles to the direction of shortening, and is inclined to the surface of the earth in a direction opposite to

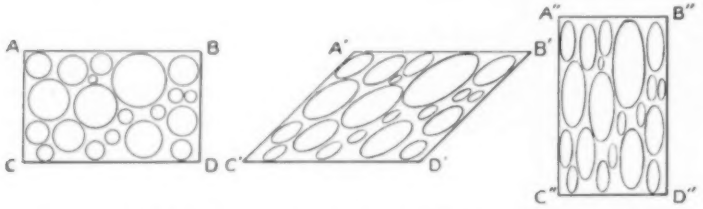


FIG. 11.— Inclined cleavage produced by shearing motion parallel to bedding without crustal shortening, and vertical cleavage produced with crustal shortening.

the direction of shortening. The forces producing a shear involve two couples, which at any given moment produce a tension in the direction of elongation and a compression in the direction of shortening. Thus, as a result of the work of the two couples in the production of cleavage by shearing parallel to the bedding, the direction of tension and the direction of shortening being inclined to the surface of the earth in opposite directions, are in such relations to each other at any given time and place that the total effect is neither elongation nor shortening of the crust of the earth.

This is illustrated by figure 11. The rectangle *ABCD* is deformed into the parallelogram *A'B'C'D'*, by shearing motion parallel to the bedding. The cleavage is parallel to the flattened ellipsoids. The area of the rectangle and parallelogram are the same, and also the line *A'B'* at the surface of the cleaved rock is of the same length as the line *AB* of the original rock before it was deformed and cleavage produced. The Ocoee slates of the Hiwassee river, west of McFarland for several miles, show a cleavage dipping to the southeast at an angle averaging about 30°. The beds are easily recognizable, and are very nearly horizontal. They show no bendings which can be dignified by the name of folds. However, even in this case the

shearing motion parallel to the surface may have been accompanied by nonrotational distortion, as a result of horizontal thrust. The deformation resulting from such stresses is shown by figure 11. In this case the rectangle *ABCD* is deformed into the rectangle $A''B''C''D''$. The shortening is here great, and yet the beds are horizontal, although thickened. In nature the two cases may be combined in any proportion. In the Hiwassee slates already mentioned close observation shows slight crenulations of the generally horizontal strata. These suggest that the shearing motion parallel to the surface has been accompanied by horizontal shortening, and that both kinds of deformation are here combined. But the relative value of each is entirely unknown, and it is therefore impossible to give any estimate of the amount of crustal shortening involved in the deformation which resulted in the cleavage described.

We therefore conclude that while monoclinal cleavage over considerable areas may involve no crustal shortening, it is probable, in most cases of such cleavage, that there is a real crustal shortening, although it is impossible to estimate its amount.

After an inclined cleavage has been produced in any region, the conditions of deformation may change as a result of denudation, and fractures may form parallel to the cleavage. These fractures may be wide apart or close together. After these partings are produced, displacements may occur similar to those of joints (Fig. 10) or they may be closed by the falling down of the overhanging material, precisely the same as in the case of ordinary normal faults. The possible elongation resulting from these secondary movements may partly or fully compensate for the earlier movements resulting in shortening.

Fissility.— Fissility is a name applied to an actual close parting of a rock which results in the production of laminæ. Fissility may possibly develop as an independent structure, although it is believed that it is commonly a structure secondary to cleavage. It is further thought that fissility generally forms as the result of ruptures along shearing planes parallel to the cleavage, from compressive rather than tensile stresses. Where

these ruptures occur close together, and there is slight differential movement, a distributive displacement may be produced, which is equivalent to a reverse fault, and which therefore results in crustal shortening, or the distributive displacements may be similar to those of the shearing joints of figure 10 and therefore result in crustal elongation. However, as in the case of cleavage just described, a region which is under compressive tangential stresses, and therefore is deformed by distributive faulting parallel to fissility, may later be under conditions of tensile tangential stresses. In this case partings will occur between the fissile laminæ, and elongation result. These elongations are strictly analogous to the elongation of normal faulting. The openings may be closed, as in the case of normal faulting, by a dropping down of the overhanging strata, or by methods of injection or cementation, as explained below.

Minute normal fault slips, secondary to cleavage or fissility, have been observed in the crystalline rocks near Blowing Rock, N. C. While during the formation of cleavage or fissility it is probable that shortening took place, it cannot be asserted that the subsequent elongation did not compensate for this, and it cannot be ascertained whether the total effect of the various deformations in this district resulted in elongation or shortening.

Vulcanism and cementation.— After a secondary structure has been produced, whether it be cleavage, fissility, joint, fault, or irregular structure, it may be taken advantage of by igneous intrusions in connection with deformation. These injections with the assistance of orogenic movements may greatly widen the openings so as to make places for great dikes. Such injections result in the local elongation of the crust of the earth. The injections may be divided into two classes, regular, approximately parallel injections, which take advantage of the above regular structures, and irregular injections.

Throughout considerable districts the amount of parallel injected materials is equal to, or surpasses the amount of original materials in which the regular secondary structures were

produced, and thus there are large extensions of the areas affected.

This is finely illustrated by many districts of the Piedmont plateau crystalline and semicrystalline rocks. A convenient district in which to see the phenomena is that of New York. In the Manhattan gneiss in the vicinity of New Rochelle, the injected material in many places surpasses in quantity the amount of the original gneiss. Parallel injection is also finely illustrated by many of the districts of pre-Cambrian gneisses in eastern Canada, and western North America. In the latter region one of the most beautiful illustrations is that of the Madison Canyon gneiss in Montana. In these regions the intrusions seem to have occurred when the rocks were rather deep-seated, and doubtless in this zone intrusions are of much greater importance than nearer the surface. However, within a few thousand feet from the surface, extensive intrusions may take advantage of joints, faults, or radiating fractures. This is illustrated by the numerous granite dikes along joints in the Sierra Nevada granite; by the dikes of Crazy Mountain, Montana;[1] by the trap dikes of the Triassic of the Connecticut valley; by the dikes of Cape Ann, Massachusetts, where, according to Shaler,[2] the dikes occupy from 5 to 10 per cent. of the surface of the country; by the dikes of western Scotland;[3] and by the dikes of many other areas.

Besides the parallel and radiating intrusions just considered, great irregular intrusions of material have occurred on a vast scale. Irregular intrusions are especially numerous among the older rocks. The injected material may occupy a large part of the surfaces of the districts affected. The irregular injections of igneous material find lateral space largely by mashing or corrugating adjacent rocks, and this causes vertical expansion of the crust. Irregular intrusives may be found in the same districts in

[1] Livingston folio, by JOSEPH P. IDDINGS and WALTER H. WEED: Geol. Atlas of the United States, No. 1, 1894.

[2] The geology of Cape Ann, Massachusetts, by N. S. SHALER: 9th Ann. Rept. U. S. Geol. Survey, 1889, pp. 579–602.

[3] Geological map of Scotland, by SIR ARCHIBALD GEIKIE.

which the parallel injections are also found. Scores of illustrations of irregular intrusions, so extensive as to occupy important or even major parts of various districts, could be mentioned of any of the great groups of rocks. A number of instances are given on pages 49–50.

All openings which may be taken advantage of by injection may also be taken advantage of by underground water deposits, and thus by a combination of fracturing and cementation the area of the rocks is increased. That the rocks may thus receive an important extension of their surface area has been noted by Shaler.[1] While parallel and irregular cementation by water solution may not be so important as igneous injection in the lateral extension of rocks, it is a more widespread phenomenon, and undoubtedly has an important effect. Wherever openings have been produced in relatively deep-seated rocks (that is, in the lower part of the zone of fracture, and in the zone of combined fracture and flowage[2]) it appears to be the rule that cementation follows them, and thus rock material again occupies the entire space.

In regions where fissility has been developed, the laminæ are cemented by layers of infiltrated material, which in many places average as wide as the laminæ cemented. This is seen at many localities in the southern Appalachians. Cementation is not more important in closing the spaces between laminæ than it is in closing joints, faults, and irregular fractures. Such cementation may be found in the same districts as the depositions along the planes of fissility, and thus double the effect, or it may occur in districts in which fissility is unimportant. The Marquette district of Michigan finely illustrates the latter. Entire formations have been broken by innumerable joints, irregular cracks, or even brecciated. The openings are now entirely closed by cementation.[3] Since the time of this cementa-

[1] The crinetic hypothesis and mountain building, by N. S. SHALER: Science, Vol. XI, 1888, pp. 280–281.

[2] Principles of North American pre-Cambrian geology, by C. R. VAN HISE: 16th Ann. Rept. U. S. Geol. Survey, Pt. I, 1896, pp. 589–594, 601–603.

[3] The Marquette iron-bearing series of Michigan, by C. R. VAN HISE and W. S.

tion, when the rocks had neared the surface by denudation, other fractures formed which have not been cemented.

Thus throughout regions in which injection or cementation is extensive, there is complete evidence of important local extension of the crust of the earth. Moreover, in the case of the igneous material, it is certain that it acts as a wedge forcing the material apart. It also is possible that the wedging effect of cementation may not be unimportant. While it is probable that upon the average the deformations which produce the fractures taken advantage of by the entering material resulted in shortening the crust of the earth, it is by no means certain that in many cases at least the extension of cementation or injection did not largely compensate for the shortening due to the deformation.

Shortening of Algonkian and Archean rocks.— No one yet has been bold enough to attempt a quantitative estimate of the shortening represented by the older mountains, the stumps of which only remain. But oftentimes it has been stated in a general way that probably the pre-Cambrian folding, and consequent shortening, is as great or greater than all subsequent folding.[1] While I am not able to disprove this conjecture, it seems to me that the closeness of corrugation assumed as general for the ancient rock is not justified by the facts. I shall separately consider the Algonkian and Archean rocks because they are so dissimilar.

In many regions the Algonkian sediments are not closely plicated. For instance, in the Lake Superior region, including the Original Huronian district, the Keweenawan and Upper Huronian sediments are very gently folded. The same statement applies to other extensive areas of pre-Cambrian sediments in Canada. The sedimentary rocks of the Adirondacks are more severely folded, but still the folding is not close. The crystal-

BAYLEY: Mon. U. S. Geol. Survey, No. XXVIII, 1896, Pls. VII, VIII, IX, XXIII, and XXVI.

[1] A criticism on the contractional hypothesis, by C. E. DUTTON: Am. Journ. Sci., Vol. VIII, 1874, p. 121. Origin of mountain ranges, by T. MELLARD READE: London, 1886, pp. 133–153.

line and semicrystalline rocks of the Blue Ridge in some places are closely folded and have secondary structures, but are in many places not closely corrugated. For instance, the quartz-schists of Tullulah mountains are in very gentle folds. The folding of the pre-Cambrian sediments in western America is also rather simple. The thick Grand Canyon series is but gently undulating. The Uinta sandstone is in a great simple arch. The thick pre-Cambrian series of Montana is gently folded. The pre-Cambrian of the Wasatch and Medicine Bow mountains are somewhat more closely corrugated, but not nearly so closely as many areas of Paleozoics in the New England region. However, there are some districts in which the folding is close and complex. This is the case with the Lower Huronian of the Vermilion and Menominee districts, and to less extent of the Marquette district, all in the Lake Superior region. The folding of the Original Laurentian district is of the most complex kind. However, even in these districts of close folding, it cannot be stated that the shortening is greater than is the case in the closely folded Tertiary rocks of the Alps.

Thus it appears that somewhat gentle folding is the rule with the pre-Paleozoic sedimentary rocks, as with the Paleozoic and post-Paleozoic, but in occasional districts the deformation, as in the later rocks, is of the most intense character. Therefore, during early geological periods, as during later geological time, orogenic movements have been concentrated along definite zones. Apparently since the beginning of Algonkian time large parts of the continents have escaped violent orogenic movements.

From the foregoing I do not mean to assert that the pre-Paleozoic sedimentary rocks are upon the average not more closely folded than later rocks. Indeed, the reverse must be the case, for the earlier rocks have partaken in subsequent foldings. The point upon which I insist is that there is no such great difference in the amount of deformation as has been thought by many.

However, it is in the Archean rocks that the apparent plications are most severe, but it is to be remembered that we have here no

criterion upon which to make an accurate judgment, as bedding is missing. As seen (p. 29), cleavage is no criterion upon which to make estimates of shortening, and this is especially true of monoclinal cleavage, and such monoclinal cleavage is found in the Archean for great distances in various places, as for instance, in the Blue Ridge and Piedmont plateau, in southwest Montana, and in various areas in Canada. Also banding is no criterion, for, as has been seen in this paper, and shown in another place,[1] the regular banding in the Archean rocks is in many cases probably due to cementation and injection. However, it is often found in these ancient rocks that the secondary structures themselves, such as slatiness and schistosity, are folded into undulations, but these are in most cases rather gentle. For instance, the schistose structure of the Blue Ridge at Doe River is a single anticline, and on the Nacoochee-Hiwassee section are two anticlines separated by a syncline. The descriptions of Emmons and King show the same simplicity of structure for the Front Range of Colorado. The undulations of the schists are so gentle that they took them to be the remains of sediments, and gave an estimate of their thickness.

Finally, wherever we find exceedingly irregular and intricate structures in which no estimate can be made of the corrugations, even of the secondary structures, we are sure to find intrusive material intricately interposed, which may itself largely or wholly compensate for the shortening which we see.

I therefore conclude with the present state of knowledge, that we are wholly unable to make any quantitative estimate of the amount of crustal shortening involved in the deformation of the Archean rocks.

Longitudinal shortening of mountain systems.—In all past estimates which have been made of shortening in mountain-making only the transverse shortening has been considered, but in order to obtain a true estimate of the effects of deformation, it is necessary to consider the amount of longitudinal shortening. If it

[1] Principles of North American pre-Cambrian geology, by C. R. VAN HISE: 16th Ann. Rept., U. S. Geol. Survey, Pt. I, 1896, pp. 662–668, 684–688.

be agreed that complex deformation is the rule instead of the exception, as I have maintained in another place,[1] it is evident that longitudinal shortening is an important factor in deformation. As a consequence of cross folds, of reverse faults, and of other cross structures, it may be that the shortening of the crust of the earth in a longitudinal direction during the mountain-making processes is as great or greater than the transverse shortening.

This becomes evident as soon as the ratios between the length and breadth of the mountain chains are considered. The Appalachian system, in its broadest sense, extends from Alabama to the St. Lawrence River, a distance of about 1300 miles. Its breadth is about 75 to 100 miles. The ratio between the length and breadth is about 15 : 1. The Cordilleran system of North America in its broadest sense extends from western Alaska to southern Mexico, a distance of about 4800 miles. The breadth varies from 100 miles at the ends of the system to 1000 miles at the middle. But where this greater breadth occurs there are considerable distances between the different chains so that the folded area is probably not more than from one-half to one-fourth of the total amount. The average width of the folded part is probably somewhere between 300 and 500 miles. Thus the ratio between the length and breadth in this case would be between 16 : 1 and 10 : 1. The Andean system extends the entire length of South America, 4500 miles. This system is a comparatively narrow one, its average width being about 200 miles. The ratio of length to breadth in this case is therefore about 22 : 1.

It thus appears in the cases of these great mountain systems, if the longitudinal shortening involves from one-tenth to one-twentieth as much shortening as the transverse deformation per mile of linear distance, that the shortening of the crust of the earth as a result of the existence of the mountain systems is as great longitudinally as transversely.

In the case of some mountain systems which have considerable breadth as compared with their length, as for instance

[1] Loc. cit., p. 626.

the Pyrenees and the Himalayas, it appears that the longitudinal shortening is relatively more important than in the mountain systems in which the ratios between length and breadth are greater, as in the cases above mentioned. In the case of the Pyrenees this is beautifully brought out by the memoirs of Roussell, which show that cross folds are here important.[1]

In the introduction of the neglected element of longitudinal shortening into the problem of crustal shortening, in mountain-making, we have a factor which, contrary to those above considered, increases the total amount of crustal shortening. In order to properly estimate the effect of the formation of a mountain system upon the area of the surface of the earth, we must know its length and breadth now, as compared with the original length and breadth of the rocks making the mountains. The amount of crustal shortening is then known in surface area, the only proper unit in which comparison can be made, for shortening along one line is of little importance unless it extends over some finite distance transverse to that line. However, the introduction of this element of longitudinal shortening very greatly complicates the quantitative estimation of the amount of shortening of the earth, and such an estimation, in a direction transverse to the mountains, as has been shown, is a sufficiently difficult task if all the factors are taken into account which should be considered.

Although aside from the purpose of this paper, it may be remarked in passing that one of the difficulties which have appeared to confront geologists is not real. Geologists, assuming that all shortening is in a direction tranverse to the length of mountain systems, have been puzzled by the resultant conclusion that the shortening of the crust of the earth is so largely concentrated in a direction transverse to the meridians.[2] When

[1] Étudé stratigraphique des Pyrenees, by JOSEPH ROUSSELL: Bull. Carte Geol de la France, Tome VI, 1893–4, and especially accompanying Pls. I to V.

Étudé stratigraphique des massifs Mantagneux du Canigou et de L'Albere, by JOSEPH ROUSSELL: Bull. Carte Geol. de la France, Tome VIII, 1896–7.

[2] A criticism upon the contractional hypothesis, by C. E. DUTTON: Am. Journ. Sci., Vol. VIII, 1874, pp. 122–123.

one recognizes that longitudinal shortening may be as important as transverse shortening, this difficulty disappears.

Finally, to know the real effect of the deformation in mountain folding it is desirable to know not only the average closeness of the plications in two directions, but the depth to which this average closeness has been observed. In short, in order to obtain the most significant estimates of the effects of crustal shortening, not one dimension of a folded mountain mass, but three, length, breadth, and depth or thickness, should be taken in account. So far as I know, two of these factors, length and depth, have been wholly ignored in all estimates of shortening. The reason for this doubtless is that the difficulties in the way of the consideration of all these factors are insuperable, at least at present.

Shortening of removed formations.—Another element of uncertainty in giving estimates of crustal shortening is the unknown shortening of the rocks which have been removed by denudation. If, as has been supposed, circumferential shortening is at a maximum at the surface of the earth, the strata which have been most deformed have been removed from time to time. It would therefore follow, if we could estimate the amount of total shortening to which the rocks of the crust have been subjected, that this amount would fall short of the real shortening which the surface of the earth had undergone. The erosive forces as they cut off the mountain tops and distribute the material upon the border of the sea, smooth out the earth's wrinkles of age.

Conclusion.—If the argument of the foregoing pages holds, it is clear that we must begin at the beginning in making estimates of the amount of crustal shortening involved in mountain-making. The published estimates have ignored so many factors which must be considered before an estimate can have any quantitative value, that I am forced to the position that they are little more than guesses. It is therefore concluded that the amount of shortening of the crust of the earth, due to its deformation, is an entirely unknown quantity. By this I do not mean to imply that the crustal shortening has not been

very great, but that as yet we can only make qualitative statements in reference to its amount; that quantitative statements are objectionable because they imply a definiteness of knowledge not warranted by the facts, and therefore stay the progress of investigation.

PART II. CAUSES OF CRUSTAL SHORTENING.

Secular cooling.— The first cause of contraction to be considered, and the only one ordinarily considered, is secular cooling. The amount of such contraction has been variously estimated. But the largest calculated amounts which the physicists will allow have always been disappointingly small to the geologists.

Mallet, on the hypothesis that the earth was liquid and had a mean temperature of 4000° F., has concluded that the earth "between its period of liquidity and its present state has shrunk in diameter by 189 miles at the least."[1] At that time, according to Mallet, the earth would have a mean radius of 4053.3 instead of 3958.8 miles. The surfaces of these spheres would be respectively about 206,457,000 and 196,942,000 square miles, and thus the surficial contraction of the earth would be about 9,515,000 square miles.

Dutton, making his calculation on another basis, concludes if the earth once had a nearly uniform temperature of 7000° F., that "if we were to assign thirty miles as the diminution of the earth's mean radius since the first formation of a cooled exterior, we should probably reach the utmost limit consistent with Fourier's theorem."[2] Taking the average radius of the earth as 3958.8 as before, the radius of the earth before contraction, according to Dutton, could not be more than 3988.8. The surface of this expanded earth would be about 199,938,000 square miles, which gives a surficial contraction of about 2,996,000 square miles.

[1] Volcanic energy: an attempt to develop its true origin and cosmical relations, by ROBERT MALLET: Trans. Roy. Soc., Vol. CLXIII, 1873, p. 205.

[2] A criticism upon the contractional hypothesis, by C. E. DUTTON: Am. Jour. Sci,. Vol. VIII, 1874, p. 121.

Fisher calculates that the radial contraction of the globe has been .65 miles since its temperature was 4000° F., and 1.9 miles if its temperature were ever 7000° F. These calculated contractions are so slight that it is not worth while to calculate the surficial contraction which would result from them.[1] Certainly, if Fisher's conclusion is approximately correct, loss of heat by secular cooling is not even an important cause for crustal shortening.

Darwin,[2] as a result of a discussion of the strains of the crust resulting from secular cooling, concludes that an earth 8000 miles in diameter would contract so that "in 10,000,000 years, 228,000 square miles of rock would be crumpled up and piled on top of the subjacent rocks."

The variation in the estimates above given is so great that the question not unnaturally arises as to whether the truth may not be far from any of them. Indeed Darwin says, with reference to his estimate, that "the numerical data with which we have to deal are all of them subject to wide limits of uncertainty."

All of the foregoing calculations as to the amount of heat lost by the earth are based upon the hypothesis that the earth has not had a higher average temperature than 7000° F. during geological time, and also on the hypothesis that the entire loss of heat is by conduction. If the present temperatures deep within the earth are to be measured by *many* thousands of degrees, as some believe, the amount of heat lost would be much greater than calculated, and the resultant contraction correspondingly important. Also the process of cooling would have been much more rapid if convectional currents assisted, by means of which the hotter material comparatively deep within the earth continued for a long time to be brought near or to the surface. It has been customary to consider the heat lost through convection as so small as to be negligible, and all calculations upon the amount of heat lost by secular cooling have ignored this quantity.

[1] Physics of the earth's crust, by OSMOND FISHER: London, 1881, p. 72.

[2] Note on C. Davison's paper on the straining of the earth's crust in cooling, by G. H. DARWIN: Phil. Trans. Roy. Soc., Vol. CLXXVIII, Pt. A, 1887, p. 249.

Loss of heat by convection is accomplished through transfers of magma and transfers of water.

If calculations are made of the loss of heat due to vulcanism, upon the basis of the volcanic materials brought to the surface of the earth at the present time, it is highly probable that the conclusion would be reached that this quantity is so small that it might be ignored. But one must remember that present vulcanism is no criterion by which to estimate the transfers of material which have resulted during past great periods of regional vulcanism. The transfers of enormous quantities of igneous material by vulcanism from deep within the earth to its outer shell or to the surface of the earth, described on page 48, strongly suggests that convectional currents have been a more important factor in the process of secular cooling that has been supposed. A large part of the heat which is carried toward the surface of the earth by magmatic convection is transferred only a part of the way by the liquid rock. For, as seen (p. 49), the intrusive rocks probably equal or exceed in quantity the extrusives, and deeper transfers may have occurred of which we have no definite knowledge. From the place where the magma is stayed the heat is brought to the surface in two ways. First, a part is transferred by conduction through the overlying mantle of rock. That such conduction occurs is shown by the fact that the temperature gradients in many districts receiving hotter material are higher than the average. Second, another part of the heat is brought to the surface by convection through underground waters. In this case the transfer of heat begun by the magma and by conduction is continued by water.

This brings us to the second agent by means of which the earth is losing heat through convectional currents. Underground water circulation everywhere pervades the outer zone of fracture at the present time, and doubtless has since a solid crust existed. That heat is brought to the surface of the earth by water is self-evident in the various districts of geysers and hot springs.

But in estimating the amount of heat which escapes by convectional transfer by underground waters, it is not sufficient to con-

sider the random hot springs. If these were the basis of calculation it would indeed be unimportant. But if underground waters upon the average reach the surface with a slightly higher temperature than when they entered it, this may be a very important means by which heat is lost through convection. While difficult or impossible to prove by observation, I think it is unquestionable that underground waters must escape at a temperature upon the average somewhat higher than that with which they entered the earth. The average temperature of water when it enters the land may be presumably taken as that of the average of the surface of the earth at that locality. From this average temperature at the surface the temperature of the rocks increases downward. The vast quantities of water which at all times is taking an underground journey gains heat as a result of its contact with the warmer rocks. At another time I shall attempt to show that the water thus heated finally reaches the surface without losing all of the heat gained in its downward course. If this be so, there is constant loss of heat.

So far as I know, no attempt has ever been made to estimate the heat lost to the earth by means of the convectional underground currents of magma and water. While I am wholly unable to prove it, I have no doubt that the absolute quantity of such heat is enormous. The heat transferred by convection is to be added to the amount transferred by conduction.

As having an effect opposite to that of conduction and convection in lowering the average temperature of the earth, it is to be noted that as a result of changing rotation period (considered pp. 54–59) heat develops within the earth in two ways. First, heat is developed by tidal friction. Darwin states that he has "calculated that the heat generated in the interior of the earth in the course of the lengthening of the day from 5 hours 36 minutes to 23 hours 56 minutes would be sufficient, if applied all at once, to heat the whole earth's mass about 3000° F., supposing the earth to have the specific heat of iron."[1] Second,

[1] On the precession of a viscous spheroid, and on the remote history of the earth, by G. H. DARWIN: Phil. Trans. Roy. Soc., Vol. CLXX, Pt. II, 1879, p. 535.

consequent upon self-condensation, as a result of increased pressures coming from the greater effectiveness of gravity as the rotation period increased, additional heat within the earth would be developed. Also condensation of the earth as a result of change of physical state (see pp. 59–61) or in any other way, would result in the development of heat.

What the residual effect of these opposite neglected factors is upon the loss of heat as ordinarily calculated, it is impossible to say, and until the various estimates of the loss of heat approach one another more nearly than they do at present, it is not worth while to make a conjecture upon this subject.

If it be true that the temperature of the interior of the earth is much higher than premised in the calculations of heat lost during secular cooling, and if the convectional movements of magma and water are important means of refrigeration, it may be that heat has been transferred to the surface from much deeper within the earth than estimated by the physicists. Dutton[1] states that below "200 or 300 miles the cooling has, up to the present time, been extremely little." Davison affirms that below 400 miles the earth has not sensibly cooled.[2] These figures are based upon the hypothesis that the loss of heat is due wholly to conduction in a globe having a uniform initial temperature of 7000° F. If this hypothesis is incorrect, the hypothetical level of no lateral stress would be at a greater depth than calculated by Reade, Darwin, and Davison, — from 2 to 8 miles.[3]

Whatever the total loss of heat as a consequence of the various positive and negative factors, if we assume a liquid earth, it is certain that all of the resultant contraction is not available

[1] A criticism upon the contractional hypothesis, by C. E. DUTTON: Am. Journ. Sci., Vol. VIII, 1874, p. 120.

[2] On the distribution of strain in the earth's crust resulting from secular cooling; with special reference to the growth of continents and the formation of mountain chains, by CHARLES DAVISON: Phil. Trans. Roy. Soc., Vol. CLXXVIII, Pt. A, 1887, p. 235.

[3] Estimates summarized in Manual of geology, by JAMES D. DANA: 4th edition, 1895, pp. 384–385.

for crustal corrugations. None of the contraction is available to explain surficial deformation until after a solid outer shell has formed, either by solidification from the center or from the surface.

Also, making no hypothesis as to the early condition of the earth, the lessening of the solid surface available for corrugation does not include the full amount obtained by calculations based upon radial contraction. So far as the exterior shell was hotter than at present, its cooling would cause circumferential contraction, and consequent lessening diameter, without crustal corrugation, just as in the case of a steel jacket which in a heated condition is put upon the core of a gun, and which upon cooling shrinks.

Furthermore, as pointed out by Davison, the outer spherical shell might continue to contract circumferentially faster than the average contraction of the interior, because nearer the surface, and more rapidly losing heat.[1] This would cause tension in the outer part of the earth, just as in the case of the jacket on the gun, which, after it has shrunk to the core, continues to contract and so firmly clasps the core as to be under great tension; or just as a large steel ingot, at a high uniform temperature, by rapid cooling may so much more rapidly contract on its outer part than in its core as to form surface tensional cracks, because of the tensile stretching during the early stages of cooling.

How important this circumferential contraction is in the case of the earth is unknown. For we do not know the average temperature of the outer shell of the earth during early geological time, nor do we know very exactly its present temperature. We can only say that certainly some quantity must be subtracted from the total surficial decrease, resulting from loss of heat, in order to obtain the amount which is available to account for crustal shortening. Estimates which disregard this correction would be true only so far back in geological time as we can assume the temperature of the outer shell to be practically the same as at present.

[1] Loc. cit., pp. 231–242.

Also (as explained p. 53), so far as heat is lost by means of vulcanism the resultant earth contraction does not give an effect in crustal corrugations in addition to that due to the transfer of the magma.

But in estimating the effect of secular cooling upon corrugation, it must be borne in mind that the earth is so large that corrugations may have begun on one part of the crust, while other parts were still subject to tension, as explained, page 46. Under these circumstances corrugations might be produced which would be compensated by tensile cracks elsewhere. Such tensile cracks may become filled with sediment, with vein material, or with igneous injections. In so far as such compensated corrugations have been produced during the early history of the earth, these deformations are in excess of the amount which it is allowable to attribute to secular cooling. Davison suggests that early in the history of the earth the continental masses might have passed earlier from the stage of tension to the stage of compression than the sea beds, and that a part of the crustal flexures of the continents were, therefore, compensated by tensile fractures under the sea.[1]

It appears from the foregoing that the corrugations of the earth due to secular cooling follow from the difference in the loss of heat by conduction and by convection, and that developed by earth movements; and from an irregular distribution of the resultant stresses, which in some places may be tensile, and at the same time in other places be compressive. Ordinarily the loss of heat by conduction only has been considered. It is clear that in secular cooling we have an important, but probably by no means an adequate, cause to account for observed crustal deformation.

Vulcanism.—The second cause to which I shall appeal to explain shell corrugations is vulcanism.

At the outset it should be said that the quantity of igneous material which now reaches the surface of the earth is no criterion by which to judge past extrusions, for at times of regional

[1] Loc. cit., p. 241.

extrusions a quantity of magma may be emitted which surpasses the entire amount emitted between epochs of regional extrusions.

To appreciate the importance of regional extrusions of magma I need only to recall the Tertiary volcanic period, during which were produced the great lava plateaus, some of them thousands of feet in thickness, in western North America, Great Britain, Iceland, Franz Josef land, New Zealand, Abyssinia, and India. In western North America the area of these volcanics is to be estimated by hundreds of thousands of square miles. The Deccan traps of India are estimated to cover 200,000 square miles and for much of this area to be from 2000 feet to 6000 feet thick.[1] But the Tertiary volcanics with which we are acquainted are only a remnant of the quantity emitted; for during Tertiary and post-Tertiary times, the erosion has been stupendous, and a large fraction of the material extruded has been converted into sedimentary rocks by means of the epigene agents.

While the volcanic rocks of the Tertiary period surpass in quantity the known igneous rocks of any previous period, it by no means follows that previous volcanic extrusions might not have been on a still vaster scale. For the further back we go, the larger is the fraction of the volcanic rocks of any given period which has been converted into sedimentary rocks by the epigene agents, and, furthermore, the proportion of the volcanics of a given period which is buried under sedimentary and igneous rocks ever increases as time passes by, so that but a small fraction of the formations bearing extrusives of great age is exposed, and in these formations, as has been seen, the larger parts of the extrusives have been destroyed.

Moreover, the extrusives are probably but the smaller part of igneous rocks. In another place I have suggested reasons why intrusives are more extensive than extrusives.[2] For the

[1] Geology of India, by R. D. OLDHAM: 2d ed., Calcutta, 1893, pp. 256, 263.

[2] Earth movements, by C. R. VAN HISE: Proc. Wis. Acad. Sci., Arts, and Letters, Vol. XI, 1898, pp. 495–496.

present purposes it is not necessary to enter into this discussion, but I wish to recall the facts as to the dominance of intrusives. Intrusive rocks are discoverable only after a region has been eroded, and it is therefore in these denuded regions that we are to look for evidence of intrusion. Beginning with the older periods, and confining our attention to America, we find that the Archean, so far as we can ascertain its original character, consists largely of modified plutonic rocks. Passing to the Algonkian, hardly an area is found in which intrusive rocks do not occupy a large percentage of the area. This is illustrated by the great masses of intrusive rocks in the Lake Superior region, which in many districts occupy large fractions of the areas. In the Rocky mountain region, in various districts, the Algonkian sedimentary rocks are subordinate to the simply enormous quantities of intrusive granite and other rocks. This is well illustrated by the Pikes Peak district, where, according to Cross,[1] the intrusive granite occupies two-thirds or three-fourths of the entire area of the one-half-square-degree quadrangle, and where the Algonkian sediments are mere fragments; by the Black Hills; by the Medicine Bow mountains, and many other ranges. Passing to the Paleozoic and Mesozoic, in almost every mountain region there are enormous masses of intrusives. This may be illustrated by the great batholiths[2] of granite in the Sierra Nevada and in the New England regions, by the laccoliths of the Henry mountains, of the Elk mountains and La Plata mountains, and by irregular intrusions, sills, and dikes, in almost every mountain district in the country. As yet the known Tertiary intrusives in America are not so important, but in Great Britain, where denudation has gone far, a vast quantity of the Tertiary intrusives has appeared. Doubtless in America also, when denudation shall have advanced far enough, correlative with the volcanics mentioned p. 48 will be found a great quantity of intrusive rocks.

[1] Pikes Peak folio, by WHITMAN CROSS: Geol. Atlas of the United States, No. 7, 1894.

[2] Suess's term *batholith* is here used in its strict etymological sense, with no reference to any theory as to how the magma was transferred, or as to whether or not it occupied previously existing spaces.

What is true of the intrusive rocks of America is true of other regions of the globe. I have selected America as an illustrative continent, because I know the facts of the field better there than elsewhere.

Any terms which one can use must fail to convey an adequate idea of the stupendous quantities of magma which have been introduced into the outer shell of the earth, or poured out upon its surface. It is clearly impossible to make even an approximate quantitative guess of the amount of igneous materials which have thus been intruded and extruded during geological times. Its quantity is certainly to be measured in tens of millions of cubic miles, rather than in smaller units.

Now in this transfer of earth material two things have happened. In so far as it has been taken from the nucleus, it has lessened its bulk. By the amount the nucleus has been lessened, the bulk of the shell has been increased.

Of this great mass which has thus migrated from the nucleus to the shell, a large proportion has stopped before reaching the surface. This is only possible by extension of the shell either vertically or laterally, or both. The forms of intrusives clearly show that both have locally occurred. Sills and laccoliths have mainly found a place to occupy by vertical extension of the shell, although to some extent lateral extension of the intruded layers (see p. 15) is also produced by them. It is equally clear that volcanic necks, dikes, and batholiths have largely found space by local lateral extension, although it is not doubted that the intrusion of such forms is also accompanied by vertical extension, and in the case of batholiths an important amount. Necks, dikes, and batholiths have formed in cracks and crevices, and wedged the walls apart, thus locally extending the crust, and giving surface which may be used in lateral mashing or corrugations elsewhere. In many cases the mashing and corrugation, and consequent thickening and vertical extension, are immediately adjacent to the intrusives. This is most marked in the case of the great batholiths. Adjacent to such enormous masses as the batholiths which are found in the Black Hills, in

the Lake Superior region, in western Massachusetts, and in Great Britain, slaty or schistose structures parallel to the intrusives are common. These structures are conclusive evidence of lateral compression and vertical extension of the rocks intruded.

As already noted, the whole of the enormous mass of the intrusives and extrusives is to be subtracted from the mass of the nucleus and added to the mass of the shell. Of these two effects the expansion of the crust is without doubt by far the more important. If the nucleus of the earth be taken as having a radius of 3900 or more miles, radial contraction of one mile would involve a loss of volume of more than 190,000,000 cubic miles. A contraction of the radius of the earth of one mile, that is, from 3958.8 to 3957.8, would give a surficial lessening of only 100,000 square miles. In the supposed case of nucleal contraction of the radius by one mile, the 190,000,000 cubic miles of material would be available for additions to the crust. If it be supposed through geological time that this amount of material has been uniformly intruded within the outer ten miles of the crust of the earth, this would demand a surface space of 19,000,000 square miles, or about one-tenth of the earth's surface. As a consequence the material previously occupying this outer shell would be crushed so as to occupy nine-tenths of its original space, and this would involve enormous lateral crustal corrugation, with consequent thickening of the outer shell from ten miles to about eleven miles.

If it be supposed that the transfer through geological time from the nucleus to the outer five miles of the crust has been only one-tenth of the amount suggested in the above paragraph, the effect would still be great. Under this supposition, the radius of the nucleus of the earth as a result of igneous intrusions has contracted one-tenth of a mile, and as a consequence its surface has been lessened by about 10,000 square miles. This would involve an intrusion into the outer five miles of the crust of the earth of about 19,000,000 cubic miles of material, and I suspect that this is an underestimate rather than an overestimate of the igneous intrusions in this outer shell of the earth. Suppos-

ing that the igneous material is uniformly distributed vertically through the outer five miles, the material would occupy a surface space of about 4,800,000 square miles, with consequent surficial contraction and thickening of the remaining material of the crust. The surficial shortening of the original crust involved would in this case be about one-half as great as that due to secular cooling throughout geological time, as calculated by Mallet, and more than one and one-half times as great as that calculated by Dutton (see p. 41), even if it were supposed that the entire contraction were available for crustal corrugation.

Of course the above figures are hypothetical. The purpose of introducing them is to show the relative importance of crustal corrugation as a result of intrusion and nucleal contraction due to the transfer of magma, and to emphasize the fact that vulcanism is probably one of the great causes for shell corrugations, for two reasons. The intrusives occupy space in the shell. The nucleus shrinks by an amount equal to the combined igneous intrusions and extrusions. I am inclined to believe that this cause for crustal deformation is of the same order of magnitude as that due to secular cooling.

The fact that periods of considerable orogenic movements generally correspond with periods of great vulcanism is very suggestive and supports the conclusion as to the importance of the above transfers of igneous material, in explaining crustal corrugations. As a single illustration of this principle of correspondence may be mentioned the fact that the great Tertiary mountain-making period in which the Sierra Nevada range was last uplifted, in which the Coast Ranges and St. Elias Alps were formed, in which the Alps themselves were produced, and in which other mountain ranges were formed, is contemporaneous with the great Tertiary period of vulcanism.

By the foregoing I do not mean to imply that vulcanism is the initial cause of the orogenic movements. The initial causes are those assigned for earth contraction. The transfers of material followed as a result of the action of the initial causes, and thus is in a measure an effect, but also where the transfer

occurs this is a further cause for crustal corrugation. Thus the transfers of magma are both effect and cause of crustal corrugations.

So far as I know, Lyell[1] was the first to suggest that there is a connection between folding and igneous intrusions. However, Fisher[2] went further than Lyell, and urged that vulcanism is the chief cause of crustal corrugation. His argument may be very briefly summarized as follows: Fissures form "through metamorphic changes. When these fissures originated below and are propagated upward, they become filled with elastic vapor, and compression results." According to Fisher, it is the expansive force of the vapor which makes the openings, and consequent corrugations, and these openings are occupied by the magma. So far as my present purposes are concerned it makes no difference how the intrusives found places for themselves. I merely insist upon the fact that somehow great spaces formerly occupied by solid rocks came to be occupied by the magma.

Shaler[3] has also appealed to igneous intrusions as a cause for mountain-making, and in a manner similar to Fisher. He thinks that in many places of New England the dikes occupy from one-twentieth to one-tenth of the superficial area.

However, neither Fisher nor Shaler consider the shrinkage of the nucleus of the earth due to the loss of the magma for both intruded and extruded materials, or the crustal corrugation which must result from this transfer of material.

In closing this part of the subject, it should be noted that crustal corrugation caused by transfers of magma involves no contraction of the earth nor lessening of its surface as a whole, except as magmatic transfer results in loss of heat by convection, as explained (p. 43). It may also be remarked that the earth contraction due to loss of heat caused by actual transfers of

[1] Principles of geology, by CHARLES LYELL: 10th ed., London, 1867, Vol. I, pp. 134–135.

[2] Physics of the earth's crust, by OSMOND FISHER: London, 1881, pp. 185–207, and pp. 284–286.

[3] The crinetic hypothesis and mountain-building, by N. S. SHALER: Science, Vol. XI, 1888, pp. 280–281.

magma to within the crust or upon its surface is not a cause for crustal corrugation in addition to that produced by the transfer itself.

Cementation.—Another cause which explains crustal corrugation is cementation (see pp. 34–35). In this process material is carried in a direction opposite to the transfers of vulcanism. In the outer zone of disintegration and decomposition material is everywhere taken into solution by underground waters, and carried to the openings below, where a part of it is deposited. Although the zone of solution which supplies the material at any time is narrow, material never fails, because this outer zone is ever migrating downward. Wherever at moderate depth during the process of deformation openings form, unless they are occupied by magma, they are gradually filled by water deposits, and thus there is local lateral extension, as in the case of vulcanism. The amount of material which thus migrates downward by means of underground waters cannot be quantitatively estimated, but it is certain that it is enormous.[1] In many regions where much deformed, comparatively deep-seated rocks have been brought to the surface, it is found that a measurable, and in some cases a considerable percentage of the entire space was once unoccupied and has been filled by cementation. The cemented rocks thus become a unit, which may be later deformed themselves, or transmit the thrusts to adjacent rocks, which may be deformed. In either case the shortening of the original material is compensated, at least in part, by the extension due to the cement, and thus the crustal corrugations are partly explained by water transfers of material.

Change of oblateness.—Peirce[2] and Darwin[3] have shown that as a result of tidal retardation the speed of rotation of the earth is decreasing, and that in the far distant past it rotated much more

[1] Earth movements, by C. R. VAN HISE: Proc. Wis. Acad. Sci., Arts, and Letters, Vol. XI, 1898, pp. 511–512.

[2] The contraction of the earth, by B. PEIRCE: Proc. Am. Acad. Arts and Sci., Vol. VIII, 1873, pp. 106–108: Reprinted in Nature, Vol. III, 1871, p. 315.

[3] On the precession of a viscous spheroid, and on the remote history of the earth, by G. H. DARWIN: Phil. Trans. Roy. Soc., Vol. CLXX, Pt. II, 1879, p. 535.

rapidly than at present, at one time possibly as fast as once in five and one-half hours. During this time of changing rotation, assuming that the geoid has accommodated itself to its period of rotation in the past as at present, Peirce states that there was a "diminution of oblateness arising from the diminished velocity of rotation upon the axis." He concludes on the hypothesis of homogeneity, when the earth rotated 4.236 times as fast as at present, that the equatorial radius would have been about 2½ per cent. greater than at present.

Taylor[1] later calculated that "when the day measured but six of our hours, the equatorial radius (assuming a true ellipsoid of revolution, and neglecting the small amount of contraction by loss of heat) would have been about one-tenth greater than it now is, or 4359 miles, and polar radius about one-sixth less, or 3291 miles. In other words, the poles would have been about 658 miles nearer the center of the earth than they are at present, and the equatorial protuberance about 396 miles higher than at present."

The discrepancy between these two results is so great that I referred the problem, for re-solution, to Professor C. S. Slichter, whose paper on this and other points immediately follows (pp. 65–78). I further asked that he obtain the amount of surficial contraction which would result from the change of oblateness. Upon the hypothesis of homogeneity, and with a period of rotation of five and one-half hours, he obtains a result which is practically the same as that of Peirce's. He finds that the earth, instead of having a mean radius of about 3959 miles, would have a polar radius of about 3736 miles, and an equatorial radius of about 4076 miles. This change from the past oblate spheroid to the present oblate spheroid would involve a contraction of the surface of the earth of about 210,000 square miles.

Change of pressure.—It further occurred to me that when the earth rotated more rapidly, the centrifugal force was greater than at present. When the rotation was four time as rapid as at

[1] On the crumpling of the earth's crust, by W. B. TAYLOR: Am. Jour. Sci., Vol. XXX, 1895, p. 257.

present the centrifugal force at the equator would be sixteen times greater than now. This being the case, it is evident that the effectiveness of gravity in producing interior pressures in the earth must have been less than at present. If the pressures were less, other things being equal, the earth would have less density than at present, and thus by a steady increase in the effectiveness of gravity during the time of decreasing rotation, we have a cause for contraction of the earth.

After reaching this qualitative conclusion, I asked Professor Slichter to handle the problem quantitatively. He finds when the period was five and one-half hours, on the hypothesis of homogeneity, that the pressure at the center of the earth was 1,688,000 atmospheres, instead of 1,772,000 atmospheres, or 4.8 per cent. less. Following Laplace's hypothesis that the earth is heterogeneous, and increases from a density of 2.7 at the surface to 10.74 at the center, and supposing that the heterogeneous oblate spheroid had an eccentricity of .4, the same as the homogeneous spheroid which has a five and one-half hour period, he finds that the pressures at the center would be 2,920,000, instead of about 3,000,000 atmospheres, or 2½ per cent. less than now. Further, as suggested by Professor Slichter, if it be supposed that during the geological history of the earth there has been a steady change from homogeneity in the direction of heterogeneity, the pressures at the centers of the spheroid, instead of increasing by the small amounts given, might have increased a much larger amount, depending upon the amount of differentiation (see Fig. 2, p. 72). The extreme case would be a change of pressure from those at the center of the homogeneous oblate spheroid when the period was five and one-half hours, that is, 1,688,000 atmospheres, to the present pressures of the heterogeneous spheroid, 3,000,000 atmospheres. In this case the pressures would have been 43.7 per cent. less than at present. It is not supposed that any such change of pressure has occurred during geological time, but the truth probably lies somewhere between this amount and the minimum, 2½ per cent., and probably much nearer the latter amount than the former.

Calculating on the basis of a heterogeneous spheroid at the beginning, *i. e.*, upon the minimum change of pressures of 2½ per cent., and assuming Laplace's laws of the relations of pressures and densities, that "The variation in pressure in the interior of the earth is proportional to the variation in the square of the density" (see p. 75), Professor Slichter finds the surface would be two-thirds of 1 per cent. greater than at present, or 1,700,000 square miles larger.

Moreover, when the surface and volume were greater than the present amounts, the effectiveness of gravity in producing pressures would be less than assumed, because of the greater size of the spheroid, so that the estimated enlargement of the surface is short of the truth. However, it does not appear practicable to make a quantitative estimate of the value of this element.

Another estimate of the amount of surficial lessening as a result of increased pressure may be made by a different line of reasoning, as follows: The most probable conjecture which can be made as to the average density which the material of the earth would have if it could all be placed under conditions of ordinary pressure and temperature is that obtained by Farrington as the average specific gravity of meteoric falls, 3.69.[1] The material of the crust probably does not represent the average composition of the earth, for differentiation must have occurred to some extent, upon any hypothesis as to the origin of the earth. All inferences as to the composition of the interior of the earth are based upon a considerable number of hypotheses, none of which are verifiable. However, the meteoric falls, not the finds, give us the density of the material which is now being added to the earth. This is probably a better guide as to the average composition of the earth than the average of meteoric finds, as suggested to me by Professor Chamberlin, because the stony falls of the past have probably largely disintegrated. Of course it is not certain that the material added at present to the

[1] The average specific gravity of meteorites, by O. C. FARRINGTON: JOURN. GEOL., Vol. V, 1897, pp. 126–130.

earth from the interplanetary spaces represents the average composition of that out of which the earth segregated, but I can see no prospect that we shall be able to make any better conjecture of the average composition than that based upon meteoric falls. As already noted, the average specific gravity of such falls is 3.69, and the specific gravity of the earth is 5.67. Now, if this increased density is due to pressure, notwithstanding the high average temperature of the interior of the earth, it follows that the volume of the earth, as a result of pressure, has been reduced in the proportion of 5.67 to 3.69. The former number is 53.65 per cent. greater than the latter. If it be supposed that this percentage of expansion in volume would be inversely as the pressure at the center, a decrease of pressure at the center of 2½ per cent. would represent an increase of volume of 1.34 per cent., and an increase of superficial area of about 1,650,000 square miles. It will be noted that the above numbers are so manipulated as to give a minimum result. They could be handled in a different way and give a larger contraction of the surface.

The correspondence of this result with that obtained by Professor Slichter, 1,700,000, by an entirely independent line of calculation, is notable and suggests that Laplace's hypothesis as to the relations of pressure and densities within the earth, and the hypothesis that the average specific gravity of the earth material at ordinary pressures and temperatures is 3.69, and that the present density of the earth, 5.67, is due to pressure, may possibly both be approximately true.

The above calculations are based upon the hypothesis that the matter of the earth remains in the same condition under all pressures. It is subsequently seen that by a change from a liquid to a solid crystalline condition there is an important contraction. The increased pressure due to lessening rotation may have carried this change further than it would otherwise have gone. Gilbert has suggested to me that a moderate change of pressure within the earth may have acted similarly to the pressure upon a spring. Until the pressure reaches a certain amount but little deformation occurs, but at a certain stage a little

added pressure produces important deformation. In another place (pp. 8–9) in this number of the JOURNAL, Powell suggests that the modulus of compressibility varies under different conditions, and that so slight a change of pressure as that due to unloading and loading by denudation, has caused important expansion and compression. If this be so, so important a change of pressure as results from change of the rotation period of the earth might have produced a more important effect upon its volume than would be obtained by supposing the modulus of compression to remain the same under all pressures.

It is not supposed that the numerical results given (pp. 56–58) for surficial lessening of the earth due to increased pressure, following upon lessened speed of rotation, approach exactness. However, it is to be noted that the numbers obtained by two different methods are concordant, and moreover, that all of the hypotheses used in obtaining these numbers have been so made as to obtain minimum results rather than maximum, and they are therefore probably much too small. It therefore appears highly probable that crustal shortening resulting from increased pressure as the speed of rotation of the earth has lessened, is one of the chief causes for earth contraction.

Change in physical condition.—Another cause of the earth's contraction is the change in the physical condition of the matter of the earth's interior. In so far as liquid material has changed to a solid amorphous material, this has produced contraction. Further, if liquid or solid amorphous material has changed to a crystalline condition, this has resulted in more important contraction.[1] This contraction is supposed to be due to the closer arrangement of the molecules. According to Delesse,[2] in passing from the crystalline to glassy state, granite decreases in density 9 to 11 per cent., syenite 8 to 9 per cent., diorite 6 to 8 per cent., dolerite 5 to 7 per cent., and trachyte 3 to 5 per cent. Barus has shown in the case of diabase, an

[1] So far as I am aware, Lyell was the first to suggest that deformation might result from a change from a liquid to a crystalline condition. (Principles of geology, by CHARLES LYELL: 10th ed., London, 1867, Vol I, pp. 134–135; Vol. II, p. 236.)

[2] See Manual of geology, by JAMES D. DANA: 4th ed., 1895, p. 265.

average rock, that it expands 13 per cent.[1] in changing from the crystalline to a liquid condition. The reverse passage from the liquid to the crystalline condition would involve a contraction of more than 12 per cent.

Even if the earth is now solid and crystalline to the center, as believed by some geologists, it by no means follows that this was the case through the major part of geological history. If the changes above mentioned have largely occurred during geological time, this has been a very important cause for contraction. However, there is no way by which the amount can be quantitatively estimated without involving so many uncertain hypotheses that it is not considered advisable to make the attempt.

Another subordinate cause for contraction is a change from less complex to more complex molecules. In so far as this change is involved in that of change from a liquid to an amorphous state or from either of these states to the crystalline condition, it has already been counted; but as a result of chemical interactions all substances, even crystalline compounds, tend to rearrange themselves under given conditions, especially where the temperature and pressure are great, so that they will have the most compact molecules. In so far as this has occurred, it is a cause for contraction, although its importance cannot be assumed to be great.

These changes in the physical state of matter and the consequent earth contraction are independent of the numerical results due to change of pressure and loss of heat given on a previous page; for all the estimates in reference to secular cooling and changing pressure are upon the hypothesis that the matter continues in the same state. The loss of heat and the increase of pressure are undoubtedly among the causes which promote change of physical condition, but in so far as change of state has occurred the resultant contraction must be added to the quantities assigned to the amounts due to secular cooling and increased pressure.

[1] The contraction of molten rock, by C. BARUS: Am. Jour. Sci., Vol. XLII, 1891, pp. 498-499.

Loss of water and gas.—Finally, as suggested by Fisher,[1] nucleal contraction may have resulted from loss of originally occluded water. Chamberlin suggests that water and gas may have been emitted which have been lost to the earth.[2] Both of these losses would result in contraction of the nucleus. Probably the quantitative value of such contraction and consequent crustal shortening is small.

General.—Doubtless as the study of the earth continues, causes other than those assigned will be discovered for crustal shortening.

However, it is believed that the cumulative effects of the various causes assigned for nucleal contraction, and for crustal corrugation, are possibly sufficient to account for the phenomena of mountain-making.

We have seen that there are four important causes for crustal corrugation. These are secular cooling, vulcanism, change of oblateness, and change of pressures. Possibly there should be included among the important causes also that of change in physical condition and cementation.

It is impossible to make any accurate quantitative comparison of the several causes. However, it is to be noted that the change in surficial area due to oblateness of 210,000 square miles is about equal to that which Darwin estimated would result from secular cooling in 10,000,000 years, 228,000 square miles. It is to be further noted that the contraction due to increased pressures at a minimum estimation, 1,700,000 square miles, is 7½ times as great as the amount which Darwin estimated would occur in 10,000,000 years as the result of secular cooling, and is therefore equivalent to the effects of secular cooling for 75,000,000 years, or for a longer period than Darwin allows for the history of the earth since the separation of the earth-moon couple. At present we are, and probably we shall long continue to be, unable to give any accurate quantitative value to the crustal shortening

[1] Physics of the earth's crust, by OSMOND FISHER: London, 1861, pp. 87, 180, 218.

[2] A group of hypotheses bearing on climatic changes, by T. C. CHAMBERLIN: JOURN. GEOL., Vol. V, 1897, pp. 656–668.

resulting from vulcanism and cementation and from change in physical condition, but it appears possible, perhaps probable (see pp. 47–52), that in vulcanism we have an explanation of as large, or even a larger, fraction of the phenomena of crustal corrugations than is furnished by any other single cause.

The various causes for crustal shortening may be divided into two classes: (1) those which involve a change in the volume of the earth; and, (2) those which involve transfers of material. (1) The loss of heat due to secular cooling, the increased pressures due to lessening rotation, and the changes of physical condition involve a contractional change of volume. Changing oblateness due to changing rotation, vulcanism, cementation and nucleal loss of water and gas, involve no appreciable change of volume. (2) Changing oblateness is only possible by deep-seated transfers of material which cause a change in the form of the earth resulting in surficial contraction. Vulcanism results in crustal expansion and nucleal contraction, and therefore in crustal corrugation. The surficial expansion due to cementation compensates for a part of the crustal corrugation. Loss of water and gas produces slight nucleal contraction, and consequently some crustal corrugation.

Furthermore, it is to be remembered that the entire effect of all these changes is available to account for crustal corrugation, with the exception of contraction due to loss of heat, which, as explained (pp. 44–45), is only partially available to account for crustal deformation. Upon the other hand, the transfers of material by vulcanism from the nucleus to within the shell has an added effect in producing crustal corrugation much greater than that due to nucleal contraction.

The crustal shortening due to changing oblateness, and increased pressures resulting from lessening rotation must have been large in the remote past. According to Darwin,[1] 56,810,000 years ago the rotation period of the earth was 6 hours 45 minutes, and 46,300,000 years ago the period was 15 hours 30

[1] On the precession of a viscous spheroid and on the remote history of the earth, by G. H. Darwin: Phil. Trans. Roy. Soc., Vol. CLXX, Pt. 2, 1897, p. 494.

minutes, *i. e.*, in about 10,500,000 years the period changed 8 hours and 45 minutes. For the entire 46,300,000 years which have since elapsed the change in period was from 15 hours 30 minutes to 24 hours or a change of 9 hours 30 minutes, but a little more than the change for the previous 10,500,000 years. At the present time, changing rotation has ceased to be a cause for mountain-making of any importance, for, according to Cayley,[1] the acceleration of the moon's motion due to tidal friction is less than 6 seconds per century.

The chief effects in mountain-making of changing oblateness and increased pressures resulting from change of rotation, as noted by Peirce in reference to the former, would be concentrated in the equatorial regions. The mountains are more numerous and higher at low latitudes than at high latitudes. The only way that this can be attributed to decreasing rotation is to suppose that the mountain-making localities were determined by the changes due to these causes, and that subsequent deformations have continued along the old zones of weakness.

However, decreasing oblateness and increasing pressures are available to explain the great deformation of the older rocks, and especially those of the Archean and Algonkian eras.

The amount of contraction which can be attributed to loss of heat is also a steadily decreasing quantity. However, in vulcanism we find a cause for crustal corrugation perhaps as potent now as at any time since the beginning of the Algonkian. Indeed, as has been seen (p. 48), the greatest volcanic epoch of which we have certain knowledge is late Tertiary time, and contemporaneous with this was the great Tertiary period of mountain-making.

It is clear that the explanation offered for crustal deformation is complex. The theory is a combined contractional and transfer theory. Moreover, the contraction, instead of being assigned to a single cause, secular cooling, is assigned to this and to increased pressure and changing physical condition. Also

[1] On the secular acceleration of the mean movement of the moon, by ARTHUR CAYLEY: Monthly Notices, Roy. Astr. Soc., Vol. XXII, 1862, pp. 171–230.

the transfers of material are of several kinds, but those of vulcanism and those of changing oblateness are the more important. The conclusions reached may therefore be considered as illustrating Chamberlin's method of multiple working hypotheses.

It is to be noted in conclusion that the argument of the above paper is independent of any theory of the origin of the earth, and of any theory of the condition of its interior, provided it is largely limited in its application to the time since the earth in some way had attained approximately its present mass. Furthermore, the contractional and corrugating effects dependent upon changing rotation involve the hypothesis that at one time the earth rotated upon its axis several times more rapidly than at present. If a more rapid rotational period be assumed than that discussed, the resultant effects would be correspondingly greater. But however the earth originated, and whatever the condition of the interior, the considerations offered which should be taken into account in estimates of crustal shortening are applicable.

C. R. Van Hise.

NOTE ON THE PRESSURE WITHIN THE EARTH.

It is the object of the present paper to briefly consider the magnitude of the pressures within the earth-spheroid, especially as influenced by the changes that have been brought about in the ellipticity of the earth's figure by its changing rotation period.

Darwin, in considering the stability of the moon-earth couple, says it seems improbable that a rotation of the earth in a little over five hours, with an ellipticity of $\frac{1}{11}$, would render the system unstable, and it hardly seems likely that better data and more perfect solution would largely affect the result, so as to make the period of revolution of the two bodies in the initial configuration very much less than five hours.[1] If the earth be assumed homogeneous throughout, as was done by Darwin in his investigations, with a density equal to the present mean density, it is a simple matter to calculate the pressures within the earth for any given eccentricity of its outer crust; and these eccentricities are, in turn, easily deducible from a knowledge of the rotation period. A table on page 327 of Part II of Thompson and Tait's Natural Philosophy gives us at once the rotation periods corresponding to various values of the eccentricity. We there find that

$e = .5$ corresponds to a rotation period of 15,730 seconds or 4½ hours.
$e = .4$ corresponds to a rotation period of 19,780 seconds or 5½ hours.

I have assumed that the separation of the moon-earth couple took place at a time when the rotation period of the earth was intermediate to the values just given, and that it would be sufficient for the purposes of geology to trace, from the epoch indicated, the changes in pressure that have taken place in the earth's interior. If it be assumed that the spheroids of eccen-

[1] Phil. Trans., 1879, Part 2, p. 536.

tricities .5 and .4 had the same volume and mass as the present earth, the polar and equatorial axes can readily be computed. Using Clark's value of the mean radius and volume, 6.3709×10^8 cm. (3958.8 mi.) and 1.0832×10^{27} cc. respectively, and Baily's value of the mean density, 5.67, I obtain the constants as given in lines 5–8 of Table I. The change of shape from the spherical

TABLE I.

		Spheroid 1	Spheroid 2	Sphere	Units
1	Eccentricity $= e$.	.5	.4	0	
2	Ellipticity $= \epsilon$ . . .	.134	.0835	0	
3	Mean radius $= a_0$	6.3709×10^8 cc.	= 3958.8 mi.		
4	Volume	1.0832×10^{27} cm.	$= 2.5988 \times 10^{11}$ cu. mi.		
5	Surface.	197,800,000	197,160,000	196,950,000	sq. miles
6	Excess of surface over that of sphere	850,000	210,000		sq. miles
7	Semi-polar axis. .	3597	3736	3959	miles
8	Semi-equatorial axis	4155	4076	3959	miles
9	Attraction at pole	995.6	990.2	981	dynes
10	Attraction at equator.	968.8	975.0	981	dynes
11	Rotation period if homogeneous . .	15730	19780		seconds
12	Centripetal acceleration at equator	106.7	66.16		dynes
13	Gravity at equator	862.1	908.8		dynes
14	Pressure at center if homogeneous	1.633	1.688	1.772	million atmospheres
15	Ratio to pressure at center of sphere	92.2	95.2		per cent.
16	Pressure at center if heterogeneous	2.88	2.92	3.00	million atmospheres
17	Ratio to pressure at center of sphere	96	97.5		per cent.
18	Change of volume, Laplacian law.	2.1	1.3		per cent.
19	Percentage change in area, Laplacian law.	1.34	8.5		per cent.
20	Actual change in area, Laplacian law.	2,700,000	1,700,000		sq. miles

form requires, of course, a change in area of surface, which change is noted in lines 5 and 6 of this table. The change in shape of the spheroid would likewise change the values of the attraction of gravitation at all points of the surface. The value of the attraction at the poles would be greater than the mean attraction on the surface of the present earth, while the attraction at the equator would be less. These values are placed in lines 9 and 10 of Table I. The determination of the attraction has been made in terms of the eccentricity from accurate formulas.[1] The values could have been computed in terms of the ellipticity[2] from the following approximate formulas, in which the square of the ellipticity has been neglected:

$$\text{attraction at pole} = (1 + \tfrac{2}{5}\,\epsilon)\, g_0$$
$$\text{attraction at equator} = (1 - \tfrac{1}{5}\,\epsilon)\, g_0$$

Here g_0 is the attraction at the surface of the same mass in spherical form.

It should be noted in this connection that the ellipticities of the spheroids under consideration are so large as to render the omission of their squares unsafe, although, as is the case in the present paper, no great importance is to be attached to the actual figures of the results. For a like reason, Clairaut's theorem may not be used with much accuracy in checking results.

Besides the reduction in the attraction at the equator due to the change in the shape of the earth, there was formerly a still further loss due to the high centripetal acceleration accompanying the short rotation period. In the case of $e = .5$, this amounted to 107 dynes, and in the case of $e = .4$ to 66 dynes; these values subtracted from the values of the attraction previously determined, give the value of equatorial gravity placed in line 13 of Table I.

The values of gravity and pressure at any point on the polar or equatorial axis of the spheroid may now be determined. If

[1] See PRATT's Figure of the Earth, 4th ed., p. 98.

[2] The ellipticity is the difference between major and minor axes divided by the major axis. I have represented it by the Greek ϵ, and have represented the eccentricity by e.

X_x represents the value of gravity at any point distant x from the center of the spheroid on an equatorial radius, and if Y_y represents the corresponding quantity for a point on the polar axis, and if g_e and g_p are the values of gravity at the equator and at the pole respectively, and if a and b are the semi-polar and semi-equatorial axes, then we have

$$X_x = \frac{g_e x}{a}; \qquad Y_y = \frac{g_p y}{b}.$$

Also if P_x and P_y represent the pressure at the same points, then

$$P_x = \frac{\rho_0}{2} g_e \left(a - \frac{x^2}{a} \right),$$

$$P_y = \frac{\rho_0}{2} g_p \left(b - \frac{y^2}{b} \right),$$

in which ρ_0 is the density of the homogeneous spheroid, and in which the other letters have the same significance as above. The following table gives the pressures at various distances from the center. The unit pressure is a million atmospheres of 10^6 dynes per sq. cm.

TABLE II.

PRESSURES WITHIN HOMOGENEOUS SPHEROIDS OF VARIOUS ECCENTRICITIES.

Distance from center along polar or equ. axis	$e = .5$	$e = .4$	$e = 0$	Distance from center along polar or equ. axis	$e = .5$	$e = .4$	$e = 0$
0	1.633	1.688	1.772	.6	1.043	1.079	1.133
.1	1.615	1.669	1.754	.7	.833	.862	.904
.2	1.567	1.618	1.700	.8	.588	.608	.638
.3	1.485	1.533	1.613	.9	.310	.321	.337
.4	1.370	1.417	1.488	1.0	.0	.0	.0
.5	1.224	1.264	1.328				

The pressures for $e = .5$ and for the sphere are shown graphically by the lower curves in Fig. 2. The line OX corresponds to either the polar or equatorial radius, as we may be pleased to consider it, but is represented, of course, as of length 10 in each case. The pressures at any other point in the spheroid can be found by drawing the equipotential surfaces; for on each of these the pressure is everywhere constant and equal, of course,

to the value of the pressure at the intersection of the equipotential surface with the polar and equatorial radii.

The pressures for $e = .4$ are not shown in the diagram, but they are not greatly different from those shown for $e = .5$. It should be noticed that the pressures for $e = .5$ are about 8 per cent. less than for the spherical form, and for $e = .4$ the pressures are about 5 per cent. less than for the spherical form.

The results above given were worked out on the supposition that the spheroid was homogeneous, having its density equal to the mean density of the earth. Of course the actual spheroid is not homogeneous, but heterogeneous, with the density increasing from surface to center. We know that the density of the surface material of the earth is approximately 2.75, and that the mean density is about twice as great. The exact law of variation of density in the interior cannot be said to be known, yet the law assigned by Laplace nearly a century ago is generally accepted as close to the truth. This law of density is as follows:

$$\rho = \frac{4.365\, a_0}{a} \sin \frac{2.4605\, a}{a_0};$$

in which ρ is the density of the stratum whose mean radius is a, the mean radius of the surface being a_0. The numerical constants are determined on the supposition that the surface density is 2.75 and the mean density twice as great. The variation according to this law is shown graphically by the heavily drawn curve of Fig. 3. An inspection of this diagram shows that the density increases quite uniformly for a considerable distance as we pass from the surface towards the center. We finally come to a central nucleus of nearly uniform density. The density at the center is 10:74.

The Laplacian law of density agrees well with the measurements of precession, and is probably as near to the truth as the measured values of the earth's mean density.

An exact method for determining the pressures within a heterogeneous spheroid without knowing its rotation period is not known to me. Even if the rotation period of the heteroge-

neous spheroids of eccentricities .5 and .4 were known, a computation of pressures would require the neglect of the squares of the ellipticities, which, in the case of ellipticities so large, would give results poorly compensating for the labor involved. I have, therefore, contented myself with two rough processes of approximation.

The pressure within a sphere in which the density is that of the Laplacian law can readily be computed by direct integration.[1] The result may be expressed as follows:

$$p = g_0 [2.7388] \left(\frac{\sin^2 n (2.4605)}{n^2} - 0.396 \right) \text{ atmospheres.}$$

In this formula, p is the pressure in atmospheres of 10^6 dynes per square cm. each, at the fractional distance n from the center of the earth, the radius being taken equal to unity for convenience. The bracket [2.7388] indicates the logarithm of a factor, and g_0 is the value of gravity at the surface.

Returning now to Fig. 1, it will be noticed that I have represented a section of the spheroid and two spheres, in contact at N. We shall suppose that the spheroid A is heterogeneous, with Laplace's law of density, and that sphere B is a sphere of same volume, same density, and the same law of density as the spheroid A. The sphere C is inscribed within the spheroid A, and has, I shall suppose, the same mean density and the same law of density as the latter. Then it is easy to see, since the law of density is such that the density increases towards the center, that the pressure at the center O of the sphere C must be less than the pressure at the center O of the spheroid A. Likewise, for the same law of density, the pressure at the point O of the sphere B is greater than the pressure at the center O of the spheroid A. The pressures at the point O within the spheres can be obtained by the formula above given, and as the pressure at the center of the spheroid is intermediate in value to those thus obtained, its value becomes approximately determined.

[1] See Osmond Fisher, Physics of the Earth's Crust, p. 32.

The pressures for the earth, calling the radius 10, are as follows, the unit being a million atmospheres:

Distance from center	Pressure in million atmospheres	Distance from center	Pressure in million atmospheres
0	3.00	6	1.25
1	2.94	7	0.85
2	2.74	8	0.50
3	2.46	9	0.21
4	2.09	10	0.00
5	1.67		

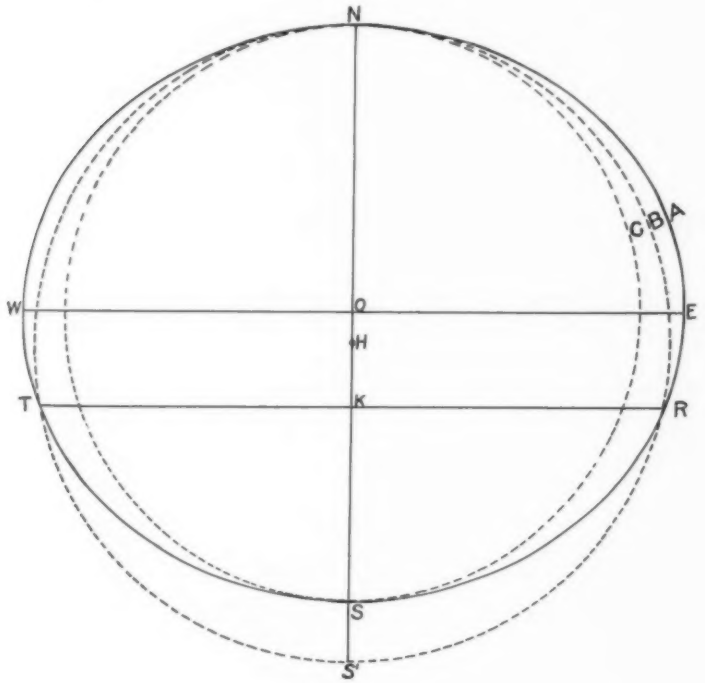


FIG. 1.

The pressures in the spheroid if e=.5 are about 4 per cent less, and if e=.4 are about 2½ per cent. less than pressures

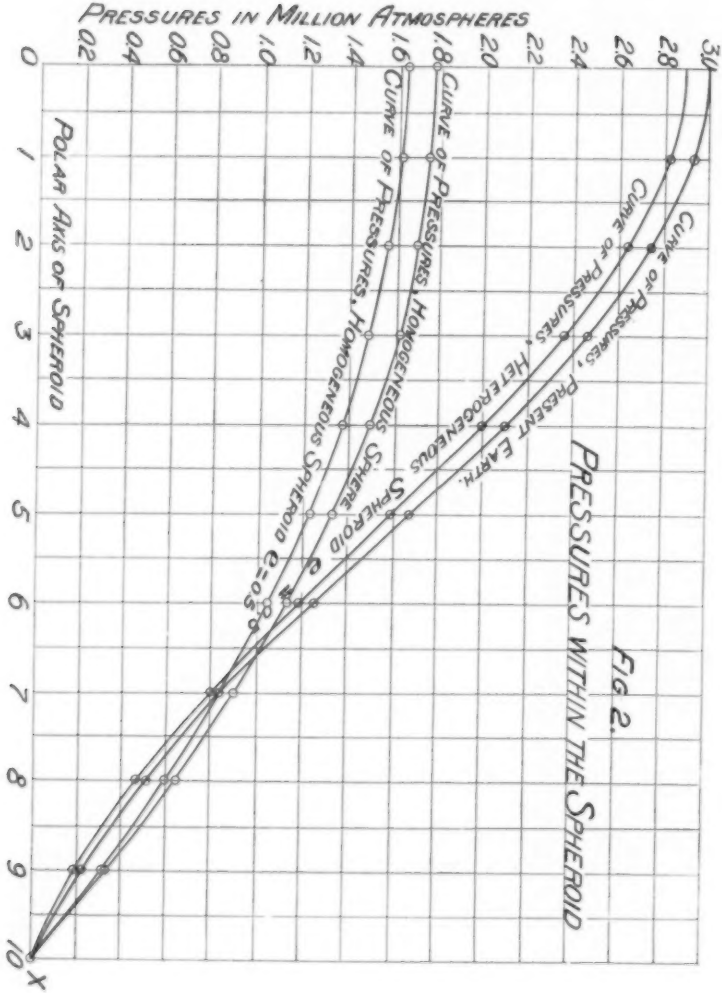

PRESSURES IN MILLION ATMOSPHERES
0.2
0.4
0.6
0.8
1.0
1.2
1.4
1.6
1.8
2.0
2.2
2.4
2.6
2.8
3.0
0
1
2
3
4
5
6
7
8
9
10
X
POLAR AXIS OF SPHEROID
CURVE OF PRESSURES, HOMOGENEOUS SPHEROID e=0.5
CURVE OF PRESSURES, HOMOGENEOUS SPHERE
CURVE OF PRESSURES, HETEROGENEOUS SPHEROID e=0.5
CURVE OF PRESSURES, PRESENT EARTH.
PRESSURES WITHIN THE SPHEROID
FIG 2.

FIG. 2.

in present earth. The pressures in the present earth and in the heterogeneous spheroid if $e=.5$ are shown graphically by the upper curves in Fig. 2. The greatest rate of change of pressures is seen to be at a point about .65 of the distance from the center to the surface.

Whatever the law of increase of density within the spheroid, provided only that the density continually increases as we approach the center, we may easily derive the following theorem:

The pressure at the center of a heterogeneous spheroid differs from the pressure at the center of the same matter in the spherical form, by a fractional amount which is less than two-thirds the ellipticity of the spheroid.

Thus if the ellipticity is .06, the pressure at the center will be not to exceed 4 per cent. less than if the matter was in the spherical form. This shows that the changes in pressure due to the changing ellipticity of the earth are limited in amount, although important, and of the same order of magnitude as the ellipticity.

Another roughly approximate method of estimating the pressures within the heterogeneous spheroid consists in assuming that all the strata of equal density have the same ellipticity as the surface. As a matter of fact, the ellipticity of the strata decrease as we approach the center by a law which may be deduced from the Laplacian law of density, and which is represented graphically by the broken line in Fig. 3. The ordinate of this curve gives the ratio of the ellipticity of a stratum to the ellipticity of the surface. It will be observed that the ellipticity of strata near the center is about 80.72 per cent. of the surface value. The actual case, then, does not differ from the assumed case of uniform ellipticity by a very large amount. It leads to the result that the change in pressure at the center of the earth due to a change in the ellipticity of the outer crust, is nearly the same in amount as if the earth were homogeneous, although the percentage change is much less than in the latter case. The relation between the ellipticity of any stratum to surface ellipticity is given by the equation :

$$\epsilon = \frac{2}{a^2}\left(1 - \frac{q^3 a^3}{3\,(1 - q\,a\,\cot q\,a)}\right)\epsilon_0$$

in which ϵ is the ellipticity of the stratum whose mean radius is a, ϵ_0 is the ellipticity of the surface, and $q = \frac{2.4605}{a_0}$ [1]

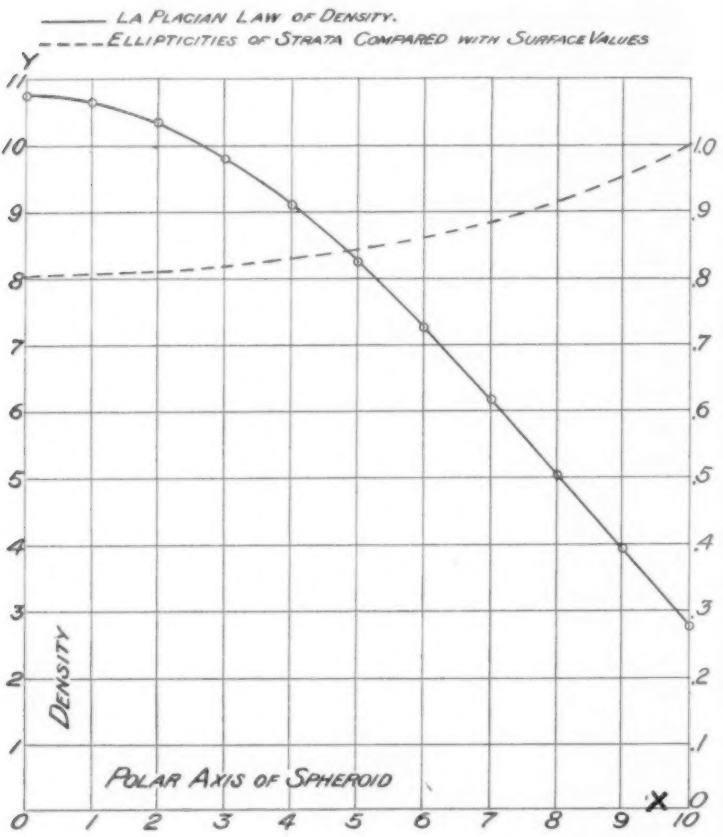

LA PLACIAN LAW OF DENSITY.
ELLIPTICITIES OF STRATA COMPARED WITH SURFACE VALUES
Y
X
DENSITY
POLAR AXIS OF SPHEROID
0 1 2 3 4 5 6 7 8 9 10
.1 .2 .3 .4 .5 .6 .7 .8 .9 1.0

FIG. 3.

[1] See Thompson and Tait, II, p. 410; Pratt, Figure of the Earth, 4th ed., p. 115; Clarke, Geodosy, p. 84.

The decrease in the size of the earth that would be brought about by the increase of internal pressure discussed above, may be computed, if it be assumed that the high density in the interior of the earth is due alone to the compressibility of matter under the enormous pressures there present. Laplace, as a matter of fact, began by assigning a law of compressibility and thence deducing the law of density; however, his law of density might be close to the truth and yet be not entirely controlled by the pressure. The equation

$$\rho = 4.365\,\frac{a_0}{a}\,\sin\,\frac{2.4605\,a}{a_0}$$

leads to the following relation connecting pressure and density.[1]

$$d\,p = k\,d\,(\rho^2)$$

or, as stated by Laplace: *The variation in pressure in the interior of the earth is proportional to the variation in the square of the density.*

The law of compressibility of gases — "Boyle's Law" — and the law of elasticity for small compressions in solids — "Hooke's Law" — state that the variation in density is directly proportional to the variation in pressure. Thus Laplace's law assumes a compressibility which is less than that given by either of the laws just named, an assumption which is, in itself, very reasonable.

From the above equation we derive

$$k\,\rho^2 = p + c$$

and determining the constants on the supposition that the surface density is 2.75, and the central density 10.74, we conclude that

$$(.02575)\,\rho^2 = p + .1947$$

in which p must be given in terms of a million atmospheres as unit. To determine the change in density due to a small change in pressure we may write,

$$\frac{d\,p}{p+c} = 2\,\frac{d\,\rho}{\rho},$$

or

$$\frac{d\,p}{p}\left(\frac{p}{p+c}\right) = 2\,\frac{d\,\rho}{\rho}.$$

[1] See O'Brien, Math. Tracts, p. 39; Pratt, p. 113; Thompson and Tait, II, p. 403.

Now if the change in pressure that has taken place is 4 per cent., we may place

$$\frac{dp}{p} = \frac{4}{100},$$

and, since c is small in comparison with p,

$$\frac{d\rho}{\rho} = \tfrac{1}{2}\,\frac{dp}{p} - n = \frac{2}{100} - n$$

in which n is a number with a small average value. Therefore, we may assume that, on the average, the change in density is about half the change in pressure. Taking the ratio as one-half, we can compute the change in volume of the spheroid for a given change in internal pressure, since, of course, volume is inversely proportional to density. I have placed results of this computation in the line 18 of Table I.

The decrease in the size of the earth due to the increase of internal pressures must likwise reduce the extent of the outer surface. If v and s represent the volume and surface of the sphere,

$$\frac{ds}{s} = \tfrac{2}{3}\,\frac{dv}{v},$$

or, for small changes, the change in surface is two-thirds the change in volume. This gives a reduction in surface amounting to about 1.3 per cent. for the spheroid $e = .5$, and .85 per cent. for the spheroid $e = .4$, or, in square miles, a reduction in surface of about 2,700,000 square miles, and 1,700,000 square miles respectively.

The compressibility of matter under high pressure and high temperature cannot be said to be known experimentally. Thompson and Tait, however, on page 415 of Part II, estimate for the average material at the surface of the earth, the compressibility that must theoretically follow from Laplace's law, and give as the result:

Melted lava, by Laplace's law - - 4.42

They also give actual experimental determinations of compressibility as follows:[1]

[1] If the radius of the earth, 6.37×10^8, be divided by each of these numbers and if

Alcohol	37.
Water	29.
Mercury	27.
Glass	5.0
Copper	8.1
Iron	4.1

The comparison, they remark, may well be considered as decidedly not adverse to Laplace's law. We may thus infer that it is far from unreasonable to hold that the high density of the interior of the earth is entirely due to the enormous pressures there present and hence it is not unreasonable to hold that the diminution in the earth's surface, pointed out above, has actually taken place. Yet, whatever future experiments may show in regard to the compressibility of melted rock, we certainly must believe that the enormous pressure at the center of the earth does have some effect, and, indeed, a large effect, in making the density high in that part of the interior.

In addition to the results which must follow from a former high rotation period of the earth and large ellipticity, important increments to the internal pressures must have taken place, if any change in the interior from homogeneity to heterogeneity has occurred. Notwithstanding the current computed results for the cooling of the earth, it seems reasonable to suppose that the energy in the interior of the earth, was, within geological times, distributed with greater homogeneity than it is at present. If any such change in the distribution of energy has taken place, then the density of the earth's interior has likewise progressed from homogeneity to heterogeneity. The curves given in Fig. 2 show that the pressure at the center of a homogeneous spheroid is only about half the pressure at the center of the present earth. Therefore, any progress that has been made

each quotient thus obtained be multiplied by 981 times the density of the substance, the result will be the *volume elasticity* in dynes per sq. cm. If the reciprocal of this last result be multiplied by 10^6, the result will be the *compression per atmosphere*. The numbers given in the table divided into the radius of the earth give what Thompson and Tait call the "lengths of the moduli of compression." See THOMPSON and TAIT, II, p. 225, § 689.

from homogeneity to heterogeneity would result in increased pressure in the interior, and in a decreased magnitude of the earth's volume and surface.

It is difficult to decide whether or not the minimum value of the eccentricity used above is too high to correspond with that uncertain date "the beginning of geological time." The rotation period of the heterogeneous spheroid would, probably, be shorter than for the homogeneous spheroid, and the shorter period may not be consistent with theories regarding the moon-earth couple. If we are required to assume a value of the eccentricity less than that used above, the changes in pressures I have given must be reduced. On the other hand, it must be remembered that there are causes at work which may augment the effects of a change in the internal pressure, and may even produce large results from what seem to be small causes. For example, if we suppose a contest in a given region in the interior between extreme heat on the one hand, and extreme pressure on the other hand, as to whether the material, or a single constituent of the material, will take on the crystalline form or not, we have a case in point. It may happen that a very slight increase in pressure may materially extend the zone in which crystallization may take place and thus result in a considerable increase in density; it is not impossible to believe that such a zone may exist in the region near the center, where pressures may be dominant on account of their enormous magnitude, and also in a region near the surface, where pressure may again be dominant owing to the lower temperature.

CHARLES S. SLICHTER.

THE GEOLOGICAL *VERSUS* THE PETROGRAPHICAL CLASSIFICATION OF IGNEOUS ROCKS.

THE rocks which make up the solid earth are of interest from a great many standpoints. A large part of geology is more or less directly concerned with them — as to their characteristics, their origin, their relationships in the mass of the earth, the changes in the rocks themselves, either metamorphism or decay, the mechanical destruction of rock masses, and structural changes in the earth dependent in many ways upon the characters of the rocks affected. From all of the points of view just indicated, and from still others, the geologist — often a specialist — has need for a nomenclature by which he may name the rocks as objects, and for various classifications expressing their observed relationships in different directions.

But while an adequate and expressive classification is a matter of great importance to many, it is commonly agreed that the systems of classification and nomenclature now in use are in a state of great confusion — of rapidly increasing confusion. To my mind the principal cause for this deplorable state of things is the lack of a clear conception of the natural relationship between *the systematic* classification of rocks, upon which their *specific* nomenclature must be based, and various *other, necessary* classifications of the same bodies. It seems that, while the multiform relations and affinities of rocks and their complex inner nature are more or less clearly understood, the futility of endeavoring to express all of these factors in one system of classification has not come sufficiently to recognition. It is my firm belief that no great progress in systematic petrography is possible until a more rational view of the relationship of that science to geology prevails among its devotees.

What is a rock? This question has often been found difficult to answer in satisfactory form. It is commonly said that

rocks are the materials which make up the crust of the earth, and to distinguish them from minerals it is pointed out that a rock is a geological body—a geological unit. But it is the unit of material or substance, and must be clearly distinguished from the geological unit of form or mass. It is the substance of the geological body, but the two conceptions are not coextensive. Chemical and mineralogical composition and structure are the chief characters of rocks as concrete objects, and it is well known that neither a stratum of sedimentary rock, nor a dike of igneous rock, is necessarily of the same composition or structure throughout. The rock unit cannot be that of the geological body as long as this is true. The rock unit is simply that which the systematic scheme of rock classification finds most desirable and practicable.

It is universally recognized that rocks not only have many relationships, when viewed from the geological standpoint, but that as objects they are extremely complex and variable in character. Lossen has said that the property of transition in all directions is an essential characteristic of the rock. It is quite possible that some who have struggled with systematic problems may be inclined to define the rock as the most variable and indefinite thing in nature! Yet rocks must be classified and named according to some system, and the task is none the less interesting or important because of the difficulties involved. The best system, that most nearly natural and logical, most uniform and stable, must ultimately prevail.

I understand that branch of geology which is concerned with rocks in all their aspects to be petrology—a treatise on rocks—and the narrower systematic, descriptive science of rocks as concrete objects, the basis for their specific nomenclature, to be petrography. This usage has now become so current in this country that discussion seems unnecessary.

Let us pass in review some of the different aspects of rocks which must be considered by the geologist, and at the same time we shall outline the field embraced by *petrology*. (1) There is the rock itself—an object of variable and complex character.

Its constitution — chemical, mineralogical, structural, and physical — must be studied and described. The differences or similarities exhibited by rocks in these respects lead to classes, groups, and lesser divisions, and the expression of these relations to a system of classification and a specific nomenclature. (2) The genesis of rocks is a subject of many phases. The source of materials, the agencies of transportation, the conditions of rock formation, each of these problems must be investigated in detail. (3) The geological occurrence embraces the formal relationships of rock masses to the earth and to each other. (4) The genetic interrelationship of rock types is one of the most difficult questions to deal with. (5) The metamorphism, and (6) the decay or destruction of rocks, each embraces a wide field. To these may be added other important lines of study. It is thus evident that petrology embraces several lines of research, each in some degree independent, each also related to the others. The results may be primarily of value as applied to the general science of geology — the history of the earth, or to the uses of the systematic descriptive science — petrography.

There has long been much discussion as to the objects of rock classification. It has been considered, on the one hand, as a mere mechanism upon which to base a nomenclature, and at the other extreme of view as a means chiefly for the expression of geological relationships of rocks. Mr. A. W. Jackson has said that nomenclature (meaning specific nomenclature) must be wholly divorced from rock classification. But that arrangement of rocks, in accordance with which they are described and their specific names are applied, is in itself the most important of all classifications, *the* systematic classification. The question is as to the criteria to be applied to produce this system. Here there must be general agreement with Mr. Jackson [1] in the proposition, often enunciated before, that a uniform and stable nomenclature must be based on facts and laws, not on theories and hypothe-

[1] On the General Principles of the Nomenclature of the Massive Crystalline Rocks, by A. WENDELL JACKSON, Amer. Jour. Sci. (3), XXIV, 1882, p. 113.

ses. Other classifications may use theoretical criteria and they will often serve useful purposes, nay, they are indeed distinctly necessary to the progress of petrology, but such arrangements must always be considered as subject to revision.

The criteria available for the systematic classification of rocks, fall into two groups, viz., the properties of the rocks themselves as objects, and their relationships to each other and to the earth, which is made up of them. There has always been conflict of views as to the use of these criteria in establishing a systematic classification. In the early years of this century there were two opposing schools, one represented by the German geologist, Werner, who classified all objects in the mineral kingdom as geological bodies, the other best represented by the French mineralogist, Haüy, to whom rocks were purely mineral aggregates. For present purposes it is not essential, however interesting, to trace the development of systematic petrography, but it is worthy of note that the geological classification of rocks is still most strongly advocated in Germany, and the mineralogical classification is still most nearly realized in France.

But the early systems of petrography failed necessarily because based upon ignorance. The material constitution of rocks was but very imperfectly known, and their geological relations were in many respects matters of crude hypothesis. All systems to the present time have failed for these reasons, and the systems of today are not free from the weaknesses due to the application of theoretical criteria.

If we review the situation as regards our present knowledge of the properties of rocks as objects it does not seem too much to say that the development of chemistry and mineralogy, and the application of the microscope to the study of rocks, have given us an accurate insight into their chemical and mineralogical composition, their structure and texture, which cannot be essentially modified by future discoveries. These are the properties universally recognized as most applicable for subclassification.

With respect to the geological relationships of rocks the case

is very different. Those rocks which are surface accumulations are so open to observation that we know many particulars of their origin—the sources of materials, the agencies by which the materials have been brought together, and the processes by which the rock has been made out of them. It is a curious fact that modern petrographers have done little toward formulating an adequate and logical classification and nomenclature for the rocks whose relationships are most evident, while they are continually extending, to an ever increasing degree of refinement, a systematic classification of igneous rocks upon foundations of theory or clear hypothesis. The fact that many criteria now used for the classification of igneous rocks are highly theoretical will hardly be questioned by anyone. From a philosophical standpoint it seems to me evident that such criteria cannot produce a stable system and must in consequence be rejected. A more detailed discussion of this question will follow.

Much has been said in recent years about the legitimate demands of geology upon systematic petrography. What are these demands? Clearly, both geology and petrography have certain reasonable demands to make, the one upon the other, but neither science has recognized its full natural rights, and hence has failed to state them properly. It is the unquestionable right of the geologist to demand of the petrographer a systematic classification of rocks, and a nomenclature expressing it, which shall be as natural and as stable as the controlling factors will allow. The petrographer must claim equal interest in such a system, and his logical counter demand is that he be allowed to construct that system through the application of the criteria best suited to produce the result desired. That is to say, he must reserve the right to reject hypotheses, theories, and even facts, if they are not adaptable.

It has often been said, from the time of the earliest classifications to the present, that rocks must be classified to express geological relationships. I believe that the geologist who today advances such a general proposition as a demand of geology upon systematic petrography is not in fact claiming his just

rights. Rather, it must be said that he does not recognize his rights. He does not perceive the true relationship between geology—petrology—and petrography. Nor does the petrographer who accepts that proposition recognize his rights.

Defining petrology and petrography, as has been done, bearing in mind the complex and variable character of the rock and its manifold relationships, it seems to me that the petrographer should esteem it his duty to produce a systematic classification of rocks with a consistent nomenclature, which shall first of all possess stability. The nearer it approaches to a natural system the better, but the character of the rock precludes the hope of securing a fully natural system. The right of the petrographer under this principle is that he may apply the test of adaptability to each criterion offered. It may be said by some that the ultimate object of petrography must be to secure a thoroughly natural classification, and that when knowledge of the rock is extensive enough such a system will be possible. I believe that that position is incorrect, if, by a natural classification, is meant one expressing all the relationships of rocks. It is not because of ignorance that we cannot set up such a natural system for rocks. The nature of the rock is the cause of this inability, not ignorance concerning it.

The petrologist must classify rocks from every standpoint. He must apply many material facts, all of which cannot possibly be used in the systematic classification of petrography, so many sided is the rock. To illustrate this point, a sandstone is a rock which may be described as inorganic, derived, compound, clastic, stratified, sedimentary, aqueous, surficial, noncombustible, etc., and each of these terms expresses a criterion that has been used in some proposed systematic classification. The petrologist must also classify some rocks on bases of theory or hypothesis, with an expressive nomenclature. For the good of his science he should be able to change such classification and dependent nomenclature as required by advancing knowledge. This amounts to a revolution if the general classification must also be revised in each case. Is it not then a logical principle, for the

good of all concerned, that the systematic classification of rocks, according to which their specific names are applied, must be based on their properties as objects, together with only such geological criteria as may be found adaptable, to the end that the system may be uniform, stable, and as natural as possible.

At this point I wish to digress for a moment and compare the task of the petrographer with that of the zoölogist, the systematic botanist or the mineralogist. From the beginnings of natural history, all natural objects have been subject to classification, at first on the most evident properties, and subsequently according to relationships. The modern zoölogist finds it possible to adopt nearly all of the general groups of animals early set up by the naturalist. Fishes, reptiles, birds, and other groups, needed only to be defined in scientific terms to bring general and scientific usage into harmony. The botanist has not been able to make his system correspond so closely to that of the naturalist. He has found that many natural groups of plants cannot be brought into his system, and he has wisely refrained from redefining the old names for those groups in such a way as to destroy their old and legitimate meaning. Thus trees (silvæ), shrubs, bushes, vines, evergreens, deciduous plants, and others, are not divisions of systematic botany, though recognized as useful and natural groups in the broader science of the vegetable kingdom. The properties and relationships of minerals may be nearly expressed in one system, but, as has been shown, rocks are of such manifold relationships that they defy a single system of classification to a much greater degree than plants.

If we now examine the schemes for the classification of rocks which have been current in the past few decades it appears that geological criteria have frequently been applied to produce the first divisions. It has been plain to all that rocks may be divided primarily into a few great classes on grounds either of geological occurrence or relationship, or of material properties. Each classification has its own justification, but the criteria to be applied in constructing a systematic scheme should plainly be those caus-

ally connected with the properties of the objects which are to be used in the further elaboration of the system. The geological agencies involved in the formation of the rock may be applied to produce rock classes differing in important material characters. This ground of classification has often been used, though not always logically carried out. It produces divisions both stable and natural. More or less distinctly the criterion of geological agency has been applied to form the classes called respectively the sedimentary, igneous and metamorphic rocks. Modern petrography has scarcely modified the old geological classification of sedimentary rocks, it has not yet anything which can be considered a system for metamorphic rocks, but it has elaborated a detailed scheme for igneous rocks, and it is now desired to review this system on the basis of the principles already presented.

Geological age has been commonly used as a criterion for the first subdivision of igneous rocks. It was originally applied in the belief that the older rocks differed in certain inherent and essential properties from younger ones. It was assumed that certain material characters were in some unexplained way governed by this geological factor, which thus became of prime classificatory value. But nearly all petrographers now perceive that assumption to have been unwarranted, and few would advocate a division of igneous rocks by age were it not for the double nomenclature in existence. It is difficult to agree upon the details of the simplification in this respect which all realize must eventually be effected.

It is in regard to the importance and applicability of geological form or place of occurrence and association of types as criteria for systematic classification that the greatest differences of opinion may be found among petrographers of today. The former of these factors, form or place of occurrence, has been and is still applied to the classification of igneous rocks on the ground that it is determinative of certain characters of rocks, and especially of structure, to a degree demanding recognition in this way. This usage is best represented by the well-known system of Prof. Rosenbusch by which massive or eruptive rocks

are divided into three great classes: "Tiefengesteine," "Ganggesteine," and "Ergussgesteine." These terms are commonly translated into English as Deep-seated rocks, Dike rocks, and Effusive rocks. Let us examine this classification from the different standpoints of the systematic petrographer and the petrologist.

To begin with it is self-evident that these class names express geological occurrence. They represent natural divisions of the geologist, they were used by him long ago and must be used in future, to express natural relationships. The geologist has a logical and practical claim upon these terms which cannot be denied. The question then is, can this geological classification be applied to the uses of systematic petrography, producing natural classes of rocks, a result which would be of great benefit to all concerned. I believe that it cannot be so used.

The system of Professor Rosenbusch is avowedly intended to meet the legitimate demands of geology upon his systematic science, as formulated by Lossen, to the effect that geological relations of rocks must be recognized as petrographical relations.[1] But while aspiring to meet the conceded demands thus expressed Rosenbusch has so redefined each of these grand divisions that it does not include all rocks belonging in it upon the criterion most clearly expressed in the name, the criterion the geologist must apply, and does include rocks that cannot logically be placed there. To illustrate by the most striking instance, the Dike rocks of Rosenbusch are not rocks occurring in dikes, which must be the geologist's definition, but rocks of as yet hypothetical derivative relation to certain other rocks. This class includes a small part of the rocks actually occurring in dikes and many not so occurring. Similarly the Deep-seated rocks of Rosenbusch are not necessarily abysmal. They appear in dikes and other intrusive bodies near the surface and even in some effusive masses. The Effusive rocks of this system occur in many intrusive masses and in the peripheries of deep-seated

[1] Über die Anforderungen der Geologie an die petrographische Systematik. Jahrbuch der K. pr. geol. Landesanstalt, 1883, p. 486.

bodies. That the above statements are true is abundantly admitted by Professor Rosenbusch in the pages of his "Mikroskopische Physiographie der massigen Gesteine."[1]

The classes of igneous rocks established by Rosenbusch upon the criterion of geological occurrence are not those of the geologist. But it is well understood by all familiar with the subject that an assumed relation between geological occurrence and structure of igneous rocks lies at the basis of Rosenbusch's inconsequent definitions of the three classes under discussion. In fact, the petrologist studying the genesis of igneous rock structures knows that they result from a complicated set of chemical and physical conditions attendant upon the consolidation of molten magmas. These conditions are as yet only partially understood. Pressure, absolute temperature, rate of cooling, the chemical changes in the fluid residue owing to fractional crystallization, the influence of so-called mineralizing agents, and several other factors, are recognized, but their relative importance is yet a matter of theory or hypothesis. A predominating influence was not long ago assigned to pressure, measured by distance from the earth's surface. But it is now known that that condition is in itself of little importance, within the zone of the earth's crust of which we have definite knowledge. It is also known that the conditions of consolidation are not controlled by either geological form or place of occurrence, to an extent capable of definite statement. The petrologist must recognize that while the typical granular structure is most common in abysmal igneous masses it may develop and is often found in the intrusive bodies of the intermediate zones of the crust and in surface masses. Nor has size of the molten body a determining influence. Neither are the porphyritic or fluidal structures dependent upon the geological form or place of occurrence of rocks exhibiting them.

Since the petrologist must inevitably apply the geological

[1] The natural limitations of the present discussion prevent an analysis of the considerations which led the German master to propose such a classification, but a review of the Rosenbusch system upon the principles here presented is now in preparation and will soon appear in this JOURNAL.

criteria of form and place of occurrence for the classification of igneous rocks into certain logical and natural groups, and has for this purpose a consistent language and mode of expression, he must surely demand of the systematic petrographer that these criteria shall not be used to produce another classification with other definitions of the same terms. Such a course produces confusion for which there can be no justification. It is to me utterly incomprehensible how the appropriation and redefinition of the geologist's terms and nomenclature can have been carried through as in the Rosenbusch system under the idea that thereby the science of geology would be benefited. And where is the science of petrography benefited by the formation of systematic groups which are confessedly unnatural and wholly unnecessary?

The petrologist must express the facts of the relationship between rock structure and geological occurrence, as between structure and other factors, to the best of his knowledge at any given time. It is not to his advantage to have this relation, always to be a matter of interpretation or theory to some degree, expressed in the systematic scheme. Nor can it be to the advantage of the system, for it will be a cause of instability.

The genetic interrelationships of igneous rocks, which most petrologists believe to be the result of what is called magmatic differentiation, are most important, but are clearly of hypothetical nature, and must remain at best matters of theory as long as the origin of the earth itself is veiled in mystery. It seems to me utterly impossible to admit such factors into the petrographic system. But it is the tendency of several leading investigators of today, notably of Rosenbusch and Brögger, to make these theoretical relationships more and more prominent in systematic classification, and from the considerations above presented I wish to make most earnest protest against this tendency as really contrary to the best interests of both geology and petrography. In thus protesting, I must not be understood as failing to appreciate the great advance in our knowledge of the origin of igneous rock varieties and of their structures, and

of the genetic relationships of types which has come within the past few years largely as a result of a promulgation of the theoretical ideas lying back of the systematic scheme advocated by Rosenbusch. One may well deny the desirability of the Dike rock group of Rosenbusch and be at the same time an ardent advocate of the theories upon which the group was established, and which have little connection with the fact of geological occurrence expressed in the name. Classifications of rocks, expressing working hypotheses as to their genesis, are necessary and may be set up at will if disconnected with the systematic classification. A stable nomenclature for rocks as objects will facilitate rather than hinder development of theoretical science.

I believe, then, that geological occurrence is not a practicable criterion for systematic classification of igneous rocks, and that it has been applied to that purpose through a misunderstanding of the true position of systematic petrography to the broader science of rocks. The petrographer has for many years failed to perceive, or, at least, to acknowledge, the right of the geologist to any special nomenclature of rocks. He has taken the time-honored terms of the geologist, redefined them to suit his own special purposes, until the geologist who is not a petrographer is almost afraid to use the simplest and most plainly denotive terms lest he be denounced as unscientific. But I maintain that it is the petrographer who has been unscientific, when he has misappropriated the natural nomenclature of the geologist, and when he has defined structural terms to express a genetic theory, or has applied them to certain rocks only out of all those possessing the structures in question. The definition of the granular structure as one having a certain mode of origin, instead of stating what the structure really is; the appropriation of *granulite* and *granite* for certain granular rocks, leaving no appropriate name for all rocks of that structure; the misuse of *porphyry* in analogous ways,—these are illustrations of thoroughly undesirable precision of definition, undesirable because at the expense of the geologist who has a prior and logical claim to these terms and needs them in their original senses.

RÉSUMÉ.—Rocks are too complex in their characters and have too many and too varied geological relationships to permit of one systematic classification expressing all their properties and relationships. A primary division on geological grounds may be carried through, producing classes of different characters, and such a division is universally advocated.

Since all the geological relationships cannot possibly be used in one system, it appears that a distinction must be made between that classification by which rocks are grouped for purposes of description and naming as concrete objects, and all other classifications. The former may be called the systematic classification, and I consider petrography to be the science presenting and applying that system to the description and naming of rocks. The broader science of petrology, using the nomenclature of petrography for specific purposes, must arrange rocks in as many other ways as are desirable to express their characters or relationships not introduced into the systematic scheme, and for the expression of these other arrangements a separate terminology is essential. It must not be appropriated under redefinition by the petrographer.

The material properties of igneous rocks afford ample criteria for establishing a systematic classification, and the use of geological relations is unnecessary. Since the geological factors of age, or of form or place of occurrence are not directly causes of the properties used in classification, they cannot be applied to produce coördinate groups. The attempts to thus apply them have been unfortunate. The justification of these attempts has been the belief that geology demanded that geological relations be recognized as petrographical relations. In the view above set forth this belief is illogical, and has resulted in injury both to geology and systematic petrography.

The impossibility of setting up an all-embracing natural classification of igneous rocks is not due to ignorance. It comes from the nature of the rock. The more we know the less shall we be able to include all relations in one classification.

WHITMAN CROSS.

ON ROCK CLASSIFICATION.

"To CLASS rightly — to put in the same group things which are of essentially the same natures, and in other groups things of natures essentially different — is the fundamental condition to right guidance of actions." These words of Herbert Spencer[1] may well form the introduction to this discussion, for they not only declare the importance of right classification, but state clearly in what it consists. It is because of the possibility of losing sight of its actual character in the approaching conflict over the reformation of rock nomenclature, that emphasis is laid at this time upon what seems to the writer to be the true character of rock classification. According to high authority[2] classification is "the act of forming a class or of dividing into classes." And further, a class is defined as "a number of objects distinguished by common characters from all others, a collection capable of a general definition."

To class rocks rightly would be to put in the same group or class those which are of essentially the same natures, or which may be distinguished by common characters from all others, and which may be capable of a general definition. This leads at once to a consideration of the nature of rocks and of the characters by which they may be distinguished from one another and which may be employed in their definition.

Nature and characteristics of rocks. — The origin or formation of rocks affects their nature to so great an extent, that it has been made a first basis for their subdivision, resulting in three categories: eruptive, sedimentary, and metamorphic. These, however, do not occupy the same position in the order of formation, but may be considered in one case as essentially primary, and in the others as secondary or derivative. The character of the rocks of these three categories, while possessing some points

[1] Man vs. State, p. 5.

[2] The Century Dictionary.

of similarity, are, nevertheless, so diverse in others, and the laws of their relationships are so unlike, that it is advisable to consider them separately. The present discussion will be confined to rocks of the first category — that is, eruptive or igneous rocks.

The most distinctive features of any igneous rock are those inherent in the mass, namely, the chemical composition, the mineral composition, and the manner in which the constituents are grouped together — the texture and physical aspect of the rock.[1] Other characters of quite as essential natures are, however, less distinctive; such are the form or dimensions of the body of the rock and its formal relations to adjacent rock masses; that is, its body as a geological unit and its occurrence or habitat, and finally its connection or relation to other igneous rocks — its association — and hence its origin.

These two groups of characters are of different orders. The first are clearly material, having to do with the chemical elements and the minerals making the substance of a rock. The second are modal, having to do with forms and relationships among rocks. Failure to recognize this difference has led to confusion of ideas and of methods in classification and in didactic treatment. The rapid development of speculation along the lines of rock genesis in conjunction with the accumulation of many facts regarding the composition, occurrence, and association of igneous rocks, has prevented the proper consideration of these phases of the science apart from one another.

It seems necessary to call attention to a difference which in this science, at least, must always exist between a classification of the material under investigation — the rocks — and a treatment for purposes of instruction or of discussion of the whole subject of petrology, or of the rocks of any petrographical province.

It may be thought at this point that the writer's conception of a classification is too narrow and restricted, and that a system

[1] Cf. MICHEL-LÉVY, A., Structures et Classification des Roches Eruptives, Paris, 1889, p. 34.

of classification may be found, such that the two objects may be accomplished at one and the same time. It is the purpose of this paper to show that this is not consistent with the nature of the case, and that a classification of rocks and the didactic treatment of them cannot be based on the same method of procedure. In order to do this it will be necessary to consider: (1) the character of the constituents of individual rocks, both chemical and mineral; (2) the results of a study of the chemical composition of all kinds of igneous rocks; (3) the occurrence of these constituents in any rock as a result of processes of differentiation, according to generally accepted theories; (4) the chemical relations of series of rocks genetically connected; (5) the nature of a rock-body or geological unit; (6) the nature of rock-association, as in petrographical provinces.

The character of the constituents of igneous rocks both chemical and mineral. — The same chemical elements occur as constituents of nearly all igneous rocks. In the great majority of those analyzed oxygen, silicon, aluminium, iron, magnesium, calcium, sodium, and potassium occur in measurable proportions; while other elements, as phosphorus, titanium, manganese, barium, strontium, chromium, nickel, cobalt, lithium, zirconium occur in determinable amounts in many rocks, and in traces in others. In many cases their presence has not been sought. In numerous cases other elements are present in very minute quantities as shown by the frequent occurrence of alla nite containing the rare elements cerium, lanthanum, didimium.[1] Neglecting for the present elements commonly occurring in very small amounts, the eight elements first named are those which chiefly characterize igneous rocks. In very few cases one or two of these may be absent or in traces only, but in almost every case they are all present. Chemical differences among these rocks consist in the proportions in which all of these elements exist in each case. Hence a classification of igneous rocks

[1] CLARKE, F. W., On the Relative Abundance of the Chemical Elements, Bull. U. S. Geol. Survey, No. 78, 1891, p. 34. Also Bull. U. S. Geol. Survey, No. 148, 1897. See also ZIRKEL, F., Lehrbuch d. Petrographie, 2d ed., 1893, Vol. I, p. 648.

upon a chemical basis in nearly every instance is the grouping together of rocks that have like proportions of the same chemical elements.

The mineral components of igneous rocks are somewhat less constant in character than the chemical elements. Rocks occur that have distinctly different mineral components such as a granite with quartz, orthoclase and biotite, and a gabbro with labradorite, augite and hypersthene. But others occur having the same kinds of minerals in quite different proportions, such as a granite with much quartz and orthoclase and little biotite, and a syenite with much orthoclase and little quartz and biotite. Neglecting for the present the rarer or least abundant minerals that are found in igneous rocks, the characteristic ones are: quartz, feldspar, leucite, nephelite, sodalites, analcite, micas, pyroxenes, amphiboles, olivine, magnetite. Of these only one possesses a fixed composition, that is, quartz. Magnetite may contain a variable amount of titanium. Leucite, nephelite, and analcite may vary in the relative proportions of potassium and sodium present. Olivines differ in the proportions of iron and magnesium. While the others represent series of minerals grouped together on crystallographic grounds, but varying often widely in chemical composition. The feldspars embrace polysilicates with variable amounts of potassium and sodium, besides compounds assumed to consist of polysilicate and orthosilicate, varying in alkali metals, calcium and silicon. Micas, pyroxenes, and amphiboles present groups which are chemically still more variable. The chief mineral components of rocks then are not definite chemical compounds, but are substances that may vary within limits according to the proportions of the chemical elements in the magma from which the rock solidified. Moreover the same chemical elements appear in several of these minerals: oxygen in all, silicon in all but one, aluminium and the alkali metals in five, iron and magnesium in four, calcium in three and so on. Hence a change in the relative proportions of the chemical elements in a magma may affect nearly all of the mineral constituents.

The results of a study of the chemical composition of all kinds of igneous rocks.— In attempting a study of the chemical composition of all kinds of igneous rocks it was found necessary at the outset to devise some means by which the variations in the chemical analyses could be compared with one another graphically or by tabulation. It was also thought desirable to consider the chemical composition in its relations to the mineral composition, as far as possible. Further it was necessary to reduce the number of variable quantities to be treated in any one scheme. And for convenience and economy of labor the oxides of the metals were employed instead of the metals themselves, in all cases their molecular proportions[1] being used and not their percentages by weight.

In order to reach a basis for the correlation of the minerals and the chemical compositions of the rocks, the minerals may be divided into two groups, one embracing quartz, feldspars and the feldspathoid minerals: leucite, nephelite, sodalite, and analcite; all except quartz containing aluminium and the alkali metals or calcium, and being free from iron and magnesium. The other group contains the amphiboles, pyroxenes, micas, olivine, magnetite; all excepting magnetite containing magnesium as well as iron. Muscovite may be classed with the first group, but grades into biotite and may also be classed with the second group. The first group includes orthosilicates of aluminium and sodium, metasilicates of aluminium, sodium, and potassium, polysilicates of aluminium, sodium, and potassium, and free silica. In each case the ratio between alumina and the alkalis is a constant 1:1; except in the sodalites, in which soda is somewhat in excess of alumina. Calcium combines with aluminium in an orthosilicate molecule in which the ratio between the alumina and lime is also 1:1. This anorthite molecule combines with albite molecules so as to form a continuous series of compounds from orthosilicate to polysilicate. A consideration of the occurrence of these minerals in igneous rocks shows that quartz

[1] Found by dividing the proportionate weights of each oxide by its molecular weight.

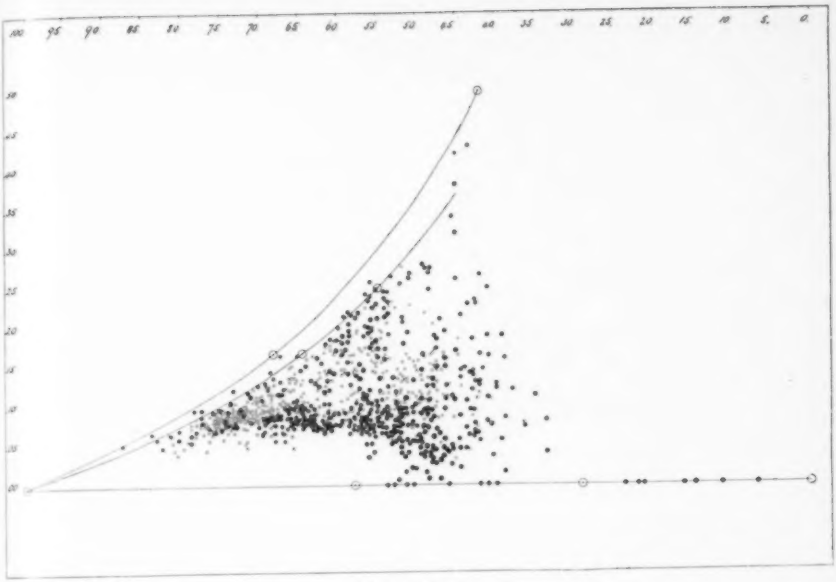


Diagram 1.

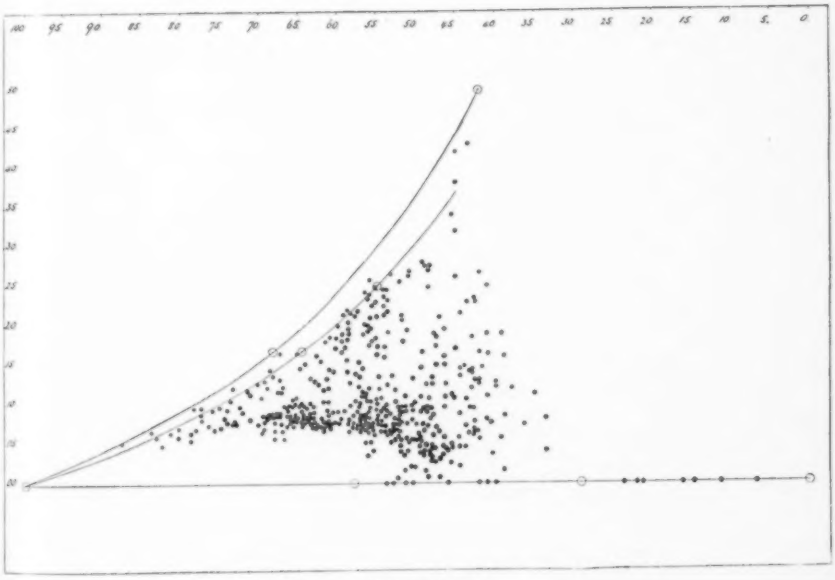


Diagram 2.

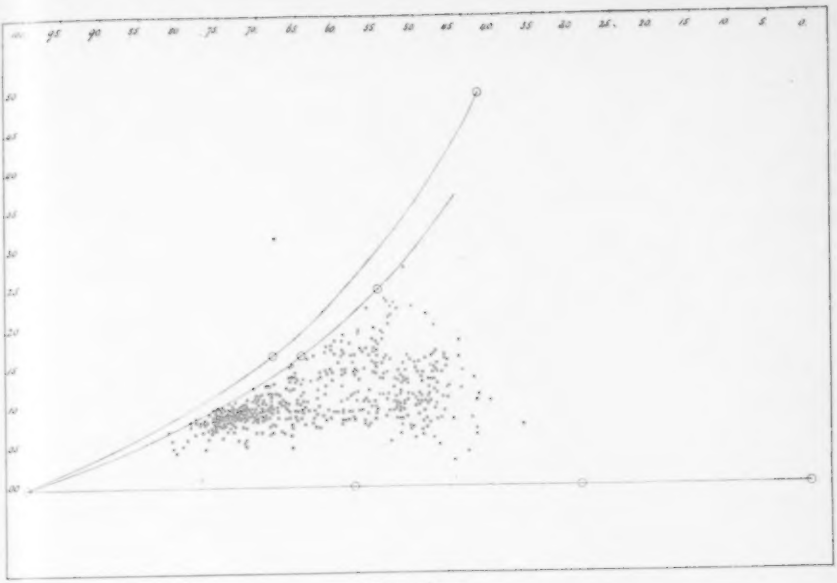


Diagram 3.

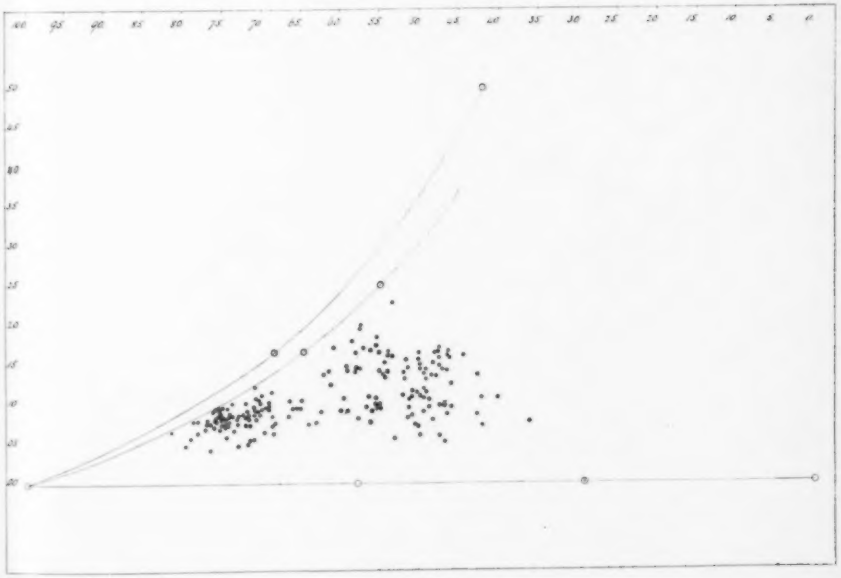
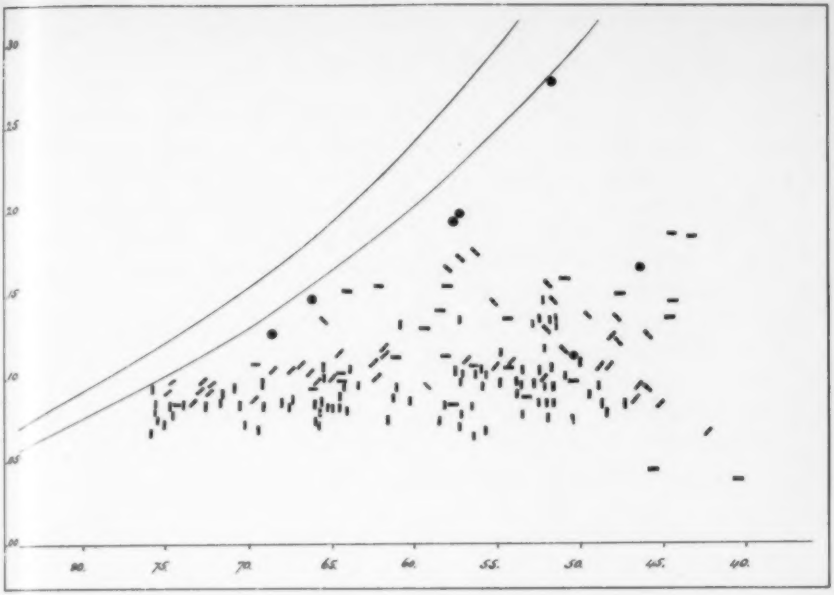


Diagram 4.



Yellowstone National Park.
Crazy Mountains, Mont.
Little Belt Mountains, Mont.
Highwood Mountains, Mont.
Bearpaw Mountains, Mont.

Diagram 5.

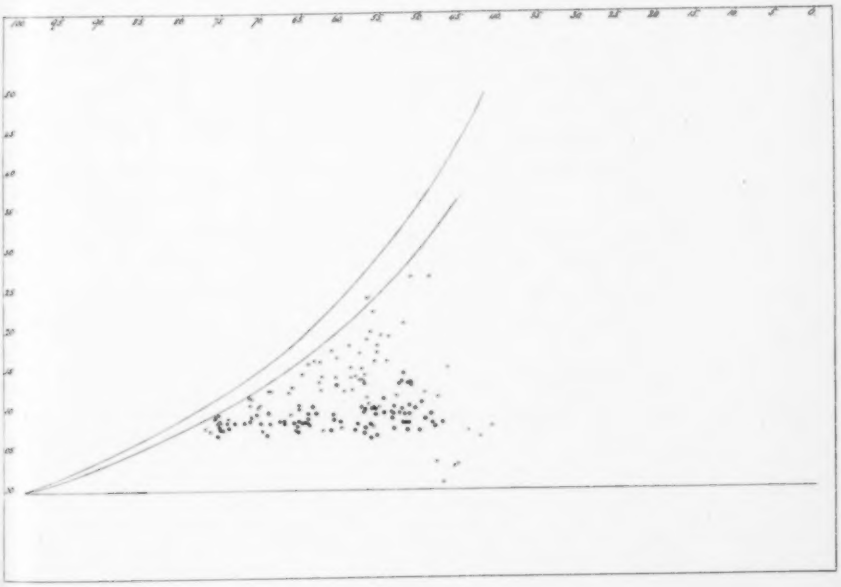


Diagram 6.

does not occur with the alkali metasilicate or orthosilicate feldspathoid minerals. And these are found in rocks comparatively low in silica, the orthosilicate being most abundant in the least siliceous rocks. But quartz is found in variable proportions with polysilicate feldspars, usually in rocks high in silica. So that it appears to be a law that so far as the alkali-aluminous silicates are concerned the highest silicate forms which is possible with the available silica in the magma. Thus a close relationship exists between the amount of silica in the magma and the development of these ortho-, meta-, and polysilicates; as does also, quite naturally, the presence of free silica or quartz. No such evident relationship has as yet been discovered between the occurrence of the ferromagnesian minerals and the amount of silica in the magma.

Investigation also reveals the law, apparently of quite general application that to a great extent the alkalis in a magma control the alumina in such a manner as to compel it to enter a feldspathic mineral if possible,[1] so that a reasonable estimate of the amount of feldspathic constituents in a rock may be formed by reckoning the alkalis and alumina together with the available silica as alkali-feldspathic constituents. And while it is well known that the alkalis may enter abundantly into micas and to a less extent into amphiboles and pyroxenes, it seems to be the rule that they do not enter the two latter minerals to any considerable extent unless the alumina present in the magma is insufficient to form feldspathic minerals. The case of biotite is different and does not conform to the rule, necessitating some modification of it.

It seems true also that in a great majority of cases aluminium does not combine with calcium to form anorthite molecules unless there is an excess of alumina over the alkalis in the magma. Exceptions to this rule, of course, occur, but its appli-

[1] This assumption which has been used by the writer in his lectures as a working hypothesis for several years has also been advocated recently by MICHEL-LÉVY in his paper entitled Classification des Magmas des Roches Éruptives, Bull. de la Soc. Géol. de France, 3d series, Vol. XXV, pp. 342–343, Paris, 1897.

cation appears to be more general than was at first suspected; and by means of both laws a crude approximation to the composition of the feldspathic constituents in any rock may be obtained.

With regard to the occurrence of the minerals of the second group, it is a general law that they are most abundant in proportion to the diminution of silica and of the feldspathic minerals, so that an expression of the variations in these will also express inverse variations in the ferromagnesian minerals. To a very considerable extent the occurrence of the orthosilicate, olivine, and of the metasilicate, hypersthene, depends on the available silica in the magma, the former occurring in rocks with lower silica.

From these considerations it is evident that a comparison of the chemical composition of rocks may be undertaken so as to bring out the relations between the alkalis and the silica, and between these components and the feldspathic constituents, and that indirectly some notion may be had of the relations of the ferromagnesian constituents. Since two coördinates or variables only can be employed in a plane diagram or table, and a third involves a representation in three dimensions, or by some other device, it is necessary to reduce the factors to at least three. Those selected for the present investigation are, first, the silica on account of its important rôle in conditioning the character of the alkali-feldspathic constituents; second, the molecular ratio between the alkalis, soda, and potash, taken together, and the silica $\left(\frac{Na_2O + K_2O}{SiO_2}\right)$, for this corresponds to the relative proportions of orthosilicate, metasilicate, and polysilicate feldspar and quartz. The amount of silica in each case was made the abscissa and the alkali-silica ratio the ordinate, and since these quantities are not of the same kind, it is not necessary that the scale of parts should be the same for both. For convenience the amounts of silica were plotted directly from the percentages in each analysis. By this means it is possible to investigate the distribution of all the rock analyses studied with

reference to these two factors. A third factor may be expressed by inserting a number at the locus in the diagram of each rock analysis which shall indicate some other molecular ratio, such as that of the approximate feldspar

$$\left(\frac{Al_2O_3-(Na_2O+K_2O)}{2\,(Na_2O+K_2O)}\right)$$

Or in another case the molecular ratio between the ferrous oxide, magnesia, and the lime not calculated with the anorthite molecule, and the silica.

$$\left(\frac{FeO+MgO+CaO-(Al_2O_3-[Na_2O+K_2O])}{SiO_2}\right)$$

The comparison by the method just indicated of 928 chemical analyses of igneous rocks has given rise to the accompanying diagrams. The analyses were selected with care in order to avoid those of much altered rocks, or those likely to be untrustworthy. The greater part have been made by chemists of the U. S. Geological Survey, which has furnished the most considerable contribution to the knowledge of the chemistry of rocks ever made by any organization. The analyses include those of all known kinds of igneous rocks that have been analyzed, so far as the writer has encountered them.[1]

Diagram 1 shows the distribution of all the analyses, the black spots representing those in which soda is more than twice the potash molecularly; the red spots those in which it is less than twice the potash. Diagram 2 is for all those in which the molecular ratio of soda to potash is greater than 2, $-\frac{Na_2O}{K_2O}>2$. Diagram 3 is for all those in which this ratio is less than 2, $-\frac{Na_2O}{K_2O}<2$. Diagram 4 is for those analyses in which this ratio is equal to or less than 1, $-\frac{Na_2O}{K_2O}\leq 1$. One of

[1] The analyses have been taken from the following sources: Analyses of Rocks and Analytical Methods, Bull. 148, U. S. Geol. Survey, 1897. Analyses in papers published by PIRSSON and WEED. ROTH's tables of rock analyses. BRÖGGER's publications, and many others.

the most notable features of Diagram 1, is the evident limitation of the range of alkali-ratios along the curved lines. These lines were suggested by the distribution of analyses in the diagram and represent in one case, the upper line, the possible range of analyses for rocks consisting wholly of silica, alumina, and soda, the two last elements being always in the proportion of 1 : 1 ; corresponding to nephelite at one extreme, succeeded by a mixture of nephelite and a possible metasilicate of these bases, or of nephelite and albite, and of albite and quartz, and finally of quartz alone. In the case of the lower line the curve corresponds to a possible range of analyses for rocks consisting of silica, alumina, and potash, the latter being in the proportion of 1 : 1. The mineral range would be from pure quartz, through a mixture of quartz and potash feldspar, to one of potash feldspar and leucite, to a possible orthosilicate of aluminium and potassium, corresponding to a potassium nephelite. In not a single instance is the sodium-aluminium limit transgressed, and only a few cases occur beyond the potassium limit. The rocks actually consist of the minerals just named. A nearly pure nephelite rock exists as urtite from the Kola peninsula,[1] and as portions of the nephelite-syenite of Dungannon township, Canada, described by Professor F. D. Adams.[2] A nearly pure albite rock has been found in California, by Mr. H. W. Turner.[3]

The fact that these limits are so closely approached, but never transgressed, is a clear indication that the alkalis do not exist in greater proportions than may satisfy this series of ortho-, meta- and polysilicate molecules. There are instances, however, in which part of the requisite alumina is replaced by ferric oxide.[4] There are a few cases in the neighborhood of

[1] RAMSAY, W.. Urtit, ein basisches Endglied der Augitsyenit-Nephelinsyenit-Serie, Geol. Fören. i Stockholm Förhandl,— Bd. 18, Häft 6, 1896, p. 459.

[2] ADAMS, F. D., On the Occurrence of a Large Area of Nepheline-Syenite in the Township of Dungannon, Ontario, Am. Jour. Sci., Vol. XLVIII, July 1894, p. 10.

[3] Further Contributions to the Geology of the Sierra Nevada, 17th Ann. Rep. U. S. Geol. Survey, Washington, 1896, p. 728.

[4] The most marked exception to this rule is the group of rocks from Leucite Hills, Wyo., described by CROSS, Igneous Rocks of the Leucite Hills and Pilot Butte, Wyoming, Am. Jour. Sci., Vol. IV, 1897, pp. 115–141.

these limits in which alkalis are in excess of the alumina, when sodalite minerals abound;[1] and others in which alumina is in excess, when muscovite forms. From the foregoing it appears that if differentiation has taken place to produce these magmas there has been no considerable accumulation of alkalis independently of all other constituents. And from the approach to the pure nephelite molecules by sodium magmas, and from the limitations of the potassium magmas in the neighborhood of the leucite molecule, there is the strongest indication that the differentiation of the magmas affected molecules more complex than simple oxides of potassium and sodium, and probably as complex as silicates of aluminium and sodium or potassium, corresponding to known mineral molecules. However, as already stated, there are proofs deducible from the laws of crystallization that the molecules in molten magmas are not stable or fixed, and the aluminium and potassium, for example, may enter orthoclase, leucite or biotite according to circumstances in the form of ortho-, meta- or polysilicate.[2]

In the direction of the maximum limit of silica, there are rocks consisting of alkali-feldspars and abundant quartz, which may grade into pure quartz, as suggested by Lehmann[3] and Howitt;[4] certain quartz veins being eruptive and extreme forms of aplitic intrusions. Thus the oxide molecule, SiO_2 appears to be capable of separation by processes of differentiation from other silicate molecules. If analyses had been made from these rocks, the diagrams would have shown a distribution of analyses as far as 100 per cent. of silica. The minimum limit of silica should occur in connection with differentiation products free

[1] Weed, W. H. and Pirsson, L. V., The Bearpaw Mountains of Montana, Am. Jour. Sci., Vol. II, No. 9, 1896, p. 197.

[2] Iddings, J. P., The Origin of Igneous Rocks, Bull. Phil. Soc., Washington, Vol. XII, 1892, p. 176.

[3] Untersuchungen über die Entstehung der altkrystallinischen Schiefergesteine, etc., Bonn., 1884, p. 55.

[4] Notes on the Area of Intrusive Rocks at Dargo, Trans. Royal Soc., Victoria, 1887, Vol. XIII, p. 152. Also Notes on Certain Metamorphic and Plutonic Rocks at Omeo, p. 10.

from silicate minerals, such as certain iron ores[1] whose analyses occur at the extreme lower right hand corner of Diagram 1. From these it is evident that molecules of iron oxides may separate by differentiation from silicate molecules. The maximum limit for soda will be when the soda-silica ratio is 0.5, that is when $NaO_2 = 21.75$ per cent. and $SiO_2 = 42.10$ per cent. The maximum limit for potash will be when the potash-silica ratio is 0.25, that is when $K_2O = 21.51$ per cent. and $SiO_2 = 54.92$ per cent. Both alkalis sink to a minimum of zero in certain kinds of peridotites, pyroxenites, and in certain eruptive iron ores. They may disappear in anorthosite composed of pure anorthite.

The limit of distribution of analyses toward low silica appears to be a line which would correspond to a variable mixture of the lowest alkali-alumina silicate — nephelite for soda magmas, and leucite for potash-magmas — and the least siliceous ferromagnesian silicate, fayalite. This limit will probably be modified when series of rocks are analyzed grading from gabbros to the iron ores. The range of variations in the proportions of iron oxides, magnesia, lime and alumina, does not appear in these diagrams.

In this connection it may be pointed out that the variations in all of the chemical constituents, other than silica, must increase in proportion as silica decreases. When 75 per cent. of the magmas is silica, only 25 per cent. remains for other constituents, but when there is only 40 per cent. of silica, 60 per cent. must consist of other compounds. It is known that rocks high in silica contain much more alumina and alkalis than other constituents, hence the variation in these other constituents cannot be great. The greatest variation occurs when part of the alumina is replaced by ferric oxide. But in rocks low in silica there is no general law controlling the other constituents, which may, therefore, vary widely in different rocks. Consequently the number of different kinds of rocks possible

[1] Vogt, J. H. L., Bildung von Erzlagerstätten durch Differentiationsprocesse in basischen Eruptivmagmata, Zeitschr. für prakt. Geol., Berlin, 1893, Jan., Apr.

for any given percentage of silica is much greater the lower the percentage of silica.

Diagram 1 shows that within the limitations above mentioned there are nearly all possible transitions in relative proportions of alkalis and silica, and the same is true for other constituents. Diagrams 2, 3 and 4 show that the range of alkali variation is greatest for rocks in which the ratio of the soda to the potash is greater than 2, and least for those in which it is less than 1. That is, it is greatest for distinctly sodic rocks and least for those very rich in potash.

While there is a clustering of analyses in the parts of the diagrams corresponding to lower alkali-silica ratios, there are no loci of specially marked aggregation, except toward the most siliceous end of the more potassic groups, and no such clustering as to suggest natural subdivisions of the analyses on any chemical basis. A chemical subdivision must be an arbitrary one, making breaks in continuous series. Comparison of Diagrams 2, 3 and 4 shows that the most siliceous rocks are generally richer in potash than in soda, also that the rocks in which alkalis decrease to nearly zero contain much more soda than potash. Those parts of the diagrams in which few analyses occur will undoubtedly be filled up as more rocks are analyzed.

It is to be noted that analyses occurring in close proximity to one another in these diagrams, and nearly alike in percentages of silica and alkalis, may in some cases differ more or less widely in other respects, so as to belong properly to different classes of rocks. They are by this method of comparison the more strongly contrasted and their essential differences more clearly recognized. This method of comparison brings closely together rocks agreeing in chemical composition, but often differing in mineral composition and physical characters, and having quite different names, and in this way emphasizes the necessity for improved definitions of names already in use and for the creation of a systematic nomenclature.

The occurrence of these constituents in any rock as a result of proc-

esses of differentiation according to generally accepted theories.—A study of the crystallization of rocks has shown[1] that different minerals and combinations of minerals may form from magmas chemically alike, according to physical conditions influencing the crystallization, proving that the chemical constituents in the molten magma do not exist, wholly at least, in definite or fixed combinations or molecules corresponding to distinct mineral molecules. Moreover, if the molecular character of a molten magma shortly before it solidifies is not fixed, but is flexible, its molecular character at an earlier period, when differentiation may take place is undoubtedly flexible or unstable, and probably to a greater degree. This is indicated by the variability in the composition of minerals of the same group, such as pyroxenes, feldspars, etc., in rocks genetically related, and assumed to have been derived by differentiation from a common parent magma. Thus in genetically related rocks the feldspars may range from those high in potash to those high in sodium and to others low in alkali metals and high in calcium; pyroxenes may range from those rich in calcium and magnesium, with little iron and aluminium, to others richer in the latter elements and to those rich in sodium and iron. Several members of either of these mineral series or groups may occur together in one rock, and may occur in varying proportions in different rocks.

The facts known regarding the mineral composition of genetically related rocks, to which the theory of differentiation has been made to correspond, are that the proportions of the component minerals, as well as their chemical compositions vary not only with different bodies of rocks, but not infrequently with one rock-body. Furthermore, this variation is in many cases gradual, with known transitions, while in other instances differences of composition are marked, and transitional forms have not yet been found. Consequently from the theory of differentiation

[1] ROTH, J., Gesteinsanalysen in tabellarischer Übersicht, etc., Berlin, 1861, p. 21.
IDDINGS, J. P., Bull. Phil. Soc. Washington, 1892, Vol. II, p. 217. Also 12th Ann. Rep. U. S. Geol. Survey, Washington, 1892, p. 659.
LINDGREN, W., A Sodalite-Syenite and other Rocks from Montana, Am. Jour. Sci., Vol. XLV, 1893, p. 297.

rocks with variable proportions of the mineral constituents are to be expected to be the rule rather than the exception And when all igneous rocks are taken into consideration all possible combinations of mineral proportions may be anticipated.

The chemical relations of series of rocks genetically connected.— The chemical characters of genetic series of rocks has been considered their most distinguishing and essential characteristic— the one quality that established their consanguinity.[1] It might be thought then that such a feature could be employed as a means of classifying igneous rocks in groups corresponding to genetic series. Careful consideration of the actual nature of this chemical character will lead to the opposite conclusion.

Certain genetic groups of rocks are distinguished by relatively high alkalis, in one instance largely potash, in another largely soda. Other genetic groups are characterized by relatively low alkalis. But even in these series not every rock contains the same high or low alkali ratio as the preponderating varieties. In fact there are often members of the series which are chemically quite different from the greater number of rocks belonging to the series. In some genetic series it is the ratio between the potash and soda that is characteristic, the total alkalis being large in some rocks of the series and low in others. But here again the ratio is never constant, and the variation may be quite large.[2] In no one genetic series yet described has any single chemical character been found to be persistent throughout the series. Nevertheless the transitions between the rocks of the series are sufficiently pronounced to leave no reasonable doubt that the rocks in question have been derived from

[1] ROSENBUSCH, H., Ueber die chemischen Beziehungen der Eruptivgesteine, Min. u. petr. Mitth., Vienna 1889, Vol. II, pp. 144–178.

BRÖGGER, W. C., Die Mineralien der Syenitpegmatitgänge der Südnorwegischen augit- und nephelinsyenite, Zeitscht. für Kryst. u. Min., Leipzig, 1890, Vol. XVI.

IDDINGS, J. P., Origin of Igneous Rocks, Bull. Phil. Soc. Washington, 1892, Vol. XII, p. 135.

[2] BRÖGGER, W. C., Die Eruptivgesteine des Kristianiagebietes. I. Die Gesteine der Grorudit-Tinguait-Serie, Christiania, 1894, 165.

WEED, W. H. and PIRSSON, L. V., Geology of Castle Mountain Mining District, Montana, Bull. U. S. Geol. Survey, No. 139, 1896, p. 137.

some parent magma by some process of chemico-physical separation or differentiation.

Besides genetic series that are chemically quite distinct from one another there are others much less so, and others that resemble one another closely. Petrographical provinces are in some cases strongly contrasted chemically. But in certain regions there appears to be a gradual shifting in chemical composition of the rocks from one locality to another. This is well illustrated in the region embracing the Yellowstone National Park, the Crazy Mountains, Castle Mountain, Little Belt Mountains, Highwood, and Bearpaw Mountains in Montana.[1] The series of rocks at each of the localities named becomes relatively richer in alkalis from the south northward, potash assuming a very prominent rôle. The analyses from these localities to the number of 175 are compared with one another in Diagram 5, which shows the gradual shifting of the alkali-silica ratios.

In general a more or less gradual shifting of chemical characters with increase in alkalis obtains for genetic series of rocks from the Great Basin in Nevada, Idaho, and Utah eastward across the Rocky Mountains to the Black Hills, S. D., Leucite Hills, Wyo., Cripple Creek, Colo., into Arkansas and southward into the Transpecos region of Texas. Throughout this vast region there are innumerable genetic series of igneous rocks. The series from somewhat remote parts of the region are chemically quite clearly distinguished, but series from intermediate localities grade into one another so that there are in reality series of series. The term series in the sense here used applies to all igneous rocks of one petrographical province that belong to one period of volcanic activity, which may be of very great

[1] IDDINGS, J. P., The Eruptive Rocks of Electric Park and Sepulchre Mountain, Yellowstone National Park, 12th Ann. Rep. U. S. Geol. Survey, 1892, pp. 569–664. Origin of Igneous Rock, loc. cit. Absarokite-Shoshonite-Banakite-Series, JOUR. GEOL., Vol. III, Chicago, 1895, p. 935.

WEED, W. H. and PIRSSON, L. V., Geology of the Castle Mountain Mining District, Montana, loc. cit. Highwood Mountains of Montana, Bull. Geol. Soc. Am., Vol. VI, Rochester, 1895, pp. 389–422. The Bearpaw Mountains of Montana, Am. Jour. Sci., Vol. I, 1896, pp. 283, 351; Vol. II, pp. 136–188. Also Bull. U. S. Geol. Survey No. 148, Washington, 1897, pp. 117–136, 142–157.

duration, embracing a whole geologic period, as, for example, the Tertiary. It may include a number of lesser series of eruptions localized within the province, as at centers of eruptions such as volcanoes; and may be highly complex, having many branchings. In some series the range of chemical variations is comparatively small, in others it is comparatively great. Examples of these appear in the rocks of the Yellowstone Park and in those of the Christiania region, whose analyses have been plotted in Diagram 6; the red spots representing those of the Christiania region.

If more limited genetic series are compared it is found that in one case the chemical variation is chiefly in the line of silica, from much to little, accompanied by abundance of feldspar molecules for the higher silica, and abundance of ferromagnesian molecules for lower silica. While in another case the chemical variation affects the silica but slightly, and shows itself in the relative abundance of alkalis and alumina on the one hand, and of ferromagnesian molecules on the other.[1] The definition of a genetic group or family of rocks, as expressed chemically, must, therefore, be very flexible and indefinite.

The natural consequence of the variability of composition among rocks of one genetic series, and of the existence of genetic series closely similar to one another chemically, is the close resemblance of some rocks of one series to certain rocks of other series. And since the differences in most cases consist in the relative proportions of the same chemical elements, it follows that some rocks of one genetic series are quite as much like certain rocks of another series as these are like other rocks of the same series. Hence, a chemical definition broad enough to cover several closely similar rocks of one genetic series may apply equally well to similar members of another genetic series, and it cannot be framed so as to exclude them. In other words, it follows from the very nature of a chemico-physical differentiation of rock magmas that some rocks belong-

[1] IDDINGS, J. P., Absarokite-Shoshonite-Banakite Series, JOUR. GEOL., Vol. III, Chicago, 1895, p. 935.

ing to different genetic series, or to different petrographical provinces, may be chemically alike.

The same is true with regard to the mineralogical characteristics of rocks. Since these are primarily dependent on the chemical composition of the magma of each rock, and also on the physical conditions attending its solidification, it follows that the minerals composing certain members of any genetic series of rocks must be like the minerals in some members of other genetic series. Definitions based on the character and proportions of these minerals must apply equally well to members of several genetic rock series.

Moreover, the texture, granularity, and physical aspect of rocks depend on both the composition of the magma and the physical conditions under which solidification took place. The latter are independent of geographical position; that is, are not localized, but are universal, and cannot be peculiar to any petrographical province. They depend on the environment of the magma in each instance, and magmas in different provinces may have existed amid similar environment and have been exposed to almost identical physical conditions.

The most distinctive features of igneous rocks, then, their chemical and mineral composition and texture, cannot serve as means of discrimination of rocks of all genetic series, consequently a group or class of rocks which may be of essentially the same natures as regards these essential qualities may embrace rocks belonging to different genetic series. It follows from this that the classing of all known igneous rocks into groups that shall have essentially the same chemical and mineral composition and texture, and which may be designated by definitions expressed in terms of these qualities, must disregard the genetic relations among the rocks of different classes.

The nature of a rock-body or geological unit.—In the foregoing discussion the term rock has been used as though applied to a mass having in each case some definite composition and texture. But it is well known that in large masses this is not always the case. In some masses the texture varies in different parts of

the whole; in others the chemical and mineral composition varies.[1] These variations may be slight and within what may be considered the limits of variation for a given class of rocks, or they may be so great as to exceed these limits, and different parts of one continuous rock-mass may have characters belonging to different classes of rocks in the usual sense. The same variations and relations exist between different parts of some rock-masses as those just mentioned as existing between some rock members of various genetic series of rocks, and for exactly the same reason. Hence, definitions of rocks based on the essential, material characters—composition and texture—cannot be made so as to discriminate always between various rock-masses.

Rock-masses of this kind are geological bodies, in that they form integral parts of the crust of the earth, such as lava streams or sheets, dikes, laccoliths, stocks, etc. Rock-masses or rock-bodies, in distinction from rocks as considered in this discussion, can not be grouped in classes on the same basis as those on which the rocks can. They may be classified according to their form or dimensions, or their mode of occurrence, or according to some general idea of composition, as homogeneity and heterogeneity.

A knowledge of the characteristics of all rock-bodies as such is necessary to a proper understanding of the mode of occurrence of igneous rocks, and is essential to any comprehensive treatment of the science of petrology.

The nature of genetic series of rocks.—The character of series of rocks that occur associated in any region so as to be considered as genetically related, differ widely in different cases. Aside from the fact that all those in one region may have solidified within the crust of the earth, while those in another region may have reached the earth's surface and have consolidated at the surface and also within the crust, there are the subsequent geological events that have transpired in each case since solidification whereby the present exposure of the rocks has been

[1] IDDINGS, J. P., Genetic Relationships among Igneous Rocks, JOUR. GEOL., Vol. I, Chicago, 1893, pp. 833–844.

brought about. This may reveal parts of all the rock-bodies originally present, or much more probably only a certain number of all once in existence, some having been entirely removed or still remaining covered. The rocks exposed in any region seldom represent the whole series of varieties that actually exist or did exist in the region. Consequently some series or associations represent comparatively few varieties of rocks, and these often quite different from one another, as when only basalt, rhyolite, and one or two varieties of andesite occur in a region, whereas other series exhibit many varieties and frequent transitions from one to another as in the dissected volcanoes in the Yellowstone Park region. Again, it is found that the range of rock varieties in some regions is limited, and in others is very wide, indicating less differentiation of the parent magma in one case than in another.

A knowledge of these associations in various regions leads to a comprehension of the laws governing the production of varieties of igneous rocks, both their probable differentiation from a parent magma, and something of the mechanism of their eruption, consequently its importance in a treatise on petrology cannot be overestimated. The consideration of these relationships is absolutely essential to a right conception of the true nature of igneous rocks.

Classification of igneous rocks and the didactic treatment of petrology.—It is hoped that the foregoing discussion has made it apparent that a systematic classification of all kinds of igneous rocks cannot be put on the same basis as a philosophical treatment of the subject-matter of petrology, which takes cognizance not only of the material character of rocks, but also of the laws governing their production, eruption, mode of occurrence, and solidification, as well as their subsequent alteration.

The object of a classification of rocks is clearly the bringing together of those that have like characters in order that they may receive a common name, and that their descriptions may be systematically arranged for convenient reference. The use of names common to all similar rocks instead of individual names, such as are given to men, is also solely for convenience; it being

considered more useful to emphasize the characters common to the rocks bearing a common name than those which might distinguish them as individuals. Moreover, it must be admitted that there is need of a systematic arrangement of descriptions of rocks, and that the descriptions of all similar rocks should be found together.

With the need of systematic classification of descriptions of rocks is an equal necessity for a systematic nomenclature. The present nomenclature is an inheritance of the most fortuitous creations of the earliest investigators, whose knowledge was of necessity often imperfect or faulty, and of recent petrographers, whose suggestions of names have been based on no uniform system. The earlier names have undergone such changes of definition as in more than one case to shift them entirely from their original application. Terms derived from geographical localities, from physical characters, or from mineral components, have been used indiscriminately. Rocks differing from one another but slightly have in some cases received totally different names, in other cases names alike except for qualifying prefixes. The time has nearly arrived for a complete reformation of petrographic nomenclature. The need is urgent but the condition of our knowledge at present is scarcely such as to warrant the immediate attempt to create a systematic nomenclature. When it is devised it should not only be sufficient for present needs, but should be capable of further development along with the growth of our knowledge of the rocks themselves.

The object of a treatise on igneous rocks should be to convey an idea of the origin and geological history of such rocks, their intricate relationships with one another, and their material characteristics. To accomplish this, the known facts may be presented in a number of ways, and the subject may be approached from different directions. Different methods of procedure may commend themselves to different writers, and may be followed with equal success. But there should be one systematic nomenclature based upon some universally accepted classification. The exact nature of this classification has yet to be determined.

JOSEPH P. IDDINGS.

EDITORIAL.

AMONG the various organizations working for the promotion of science, the state academies of science have recently become prominent, particularly in the middle west. The Ohio, Indiana, Wisconsin, Minnesota, Iowa, Nebraska, and Kansas academies are doing an important and highly valuable work. In most cases their meetings are held at some time during the winter holidays, and they bring together a large proportion of the influential men of science of their respective states. Our country is a large one and to many a scientific worker the pilgrimage eastward to the winter meeting of his special society is something only to be undertaken after careful consideration of ways and means. To such a one the state academy is a real boon, and in any event the coming together of men of scattered but related fields of work is perhaps as productive of good as the meeting of specialists alone.

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THE state academy is, however, making a peculiar place for itself aside from serving as a meeting place for various scientific workers. It is rapidly becoming a molder of public opinion on the numerous semi-public scientific questions which are constantly springing up. It is the adviser of the legislature in matters of scientific work and its potency in that direction has been instanced in the part taken by the Wisconsin and Iowa academies in securing the legislation necessary for geological and natural history surveys. In several of the states this semi-public position of the academy has been recognized, and the state has undertaken to publish the proceedings.

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IN the recent meetings of the state academies the geologists have as usual taken an active part. The Minnesota meeting,

being the twenty-fifth anniversary, was celebrated by a very attractive programme. Papers were read by Winchell, Grant, Sardeson, Berkey, and others upon various geological problems. The Iowa programme included papers by Todd, Calvin, Keyes, Udden, Sardeson, Bain, Leverett, and others and was particularly interesting to students of the drift. The Wisconsin programme embraced papers by Van Hise, Collie, Hobbs, Lurton, Slichter, with a report of progress of the Geological and Natural History Survey by Birge and Marsh. The Indiana programme was also a strong one, and the Nebraska meeting, held some time earlier, was well attended. The publication of the proceedings of these meetings will be awaited with interest. H. F. B.

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Messrs. Spring and Rowland have recently communicated to the Belgium Academy a series of observations made upon the amount of carbonic acid contained in the atmosphere during the year. They give the result of 266 determinations made in the city of Liege, Belgium, on one side of which there is an industrial and on the other an agricultural district. The average amount of carbonic acid contained in ten thousand parts of air was 5.1258 parts by weight and 3.3526 parts by volume. These gentlemen remark that this is more than the amount contained in the air of Paris, which is 4.831 parts by weight and 3.168 by volume. The large amount at Liege is owing not only to the large iron works there but also to the fact that the city is surrounded by coal mines. To this the authors attribute the greater heat of the city as it is well known that a small amount of carbonic acid in the air causes the absorption and prevents the radiation of heat. They also attribute the cold weather of May to the diminution of the carbonic acid caused by its consumption by the exceptionally vigorous growth of leaves at that season. Their observations show that a fall of snow will increase the amount to 3.761 by volume and that in cloudy weather the amount was 3.571 parts and that there was always a larger amount in winter than in summer. They also found that the amount was diminished by high winds but increased

with a high barometer. (Egleston Thomas, *Trans. Am. Soc. Civil Engineers*, Vol. XV, p. 650, New York, 1886.) H. F. B.

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THE attendance at the Montreal meeting of the Geological Society of America was small, but the session was both pleasant and profitable. The hospitality of the people of Montreal, always cordial, was notable for its warm-heartedness. The interest and importance of the papers compared very favorably with those of the preceding sessions, and in several notable instances possessed peculiar interest. The presidential address of Dr. Orton was an admirable exposition of the theoretical aspects of petroleum and was made attractive not less by the charm of its manner than by the judiciousness of its matter. The pleistocene papers as usual led in number, but the preponderance was but slight. The petrological papers followed them closely in number and perhaps surpassed them in fullness and in the labor of preparation. Two of them were especially notable for their careful elaboration. It was gratifying to observe a relative increase in palæontological papers. It has been a source of deep regret to many American geologists that palæontologists have in recent years so largely drifted in the direction of phylogeny (important as that is) and that the study of faunas and floras as such and as factors in the history of the earth has not found larger expression in the papers of the Geological Society. The evolution and migration of faunas and floras is comparable if not superior in importance and interest to the evolution of species and families, and merits an ample share of palæontological effort. Papers upon regional geology, which have always constituted a notable proportion of the programme, held a fair place at the Montreal meeting. The dynamical papers were few, but the peculiar interest which attached to the experiments of Dr. Adams on the flow of rocks under pressure made full atonement for the scantiness of their numbers. The important paper of Professor Van Hise on "Shortening of the Outer Part of the Earth" was read by title only. Physiographical papers which have sometimes occupied a prominent

place on the programme found notable expression in but a single presentation.

The following is a list of the titles of the papers offered. Abstracts of a portion of these appear in this number of the JOURNAL and others will follow:

"Notes on the Sands and Clays of the Ottawa Basin." By R. W. ELLS.

"Topography and Glacial Deposits of the Mohawk Valley." By ALBERT P. BRIGHAM.

"Topography and History of Jamesville Lake, N. Y." By EDMUND C. QUEREAU.

"Location and Form of a Drumlin at Barre Falls, Mass." (By title.) WILLIAM H. NILES.

"Drift Phenomena of the Puget Sound Basin." (By title.) By BAILEY WILLIS.

"Niagara Gorge and St. David's Channel." By WARREN UPHAM.

"Notes on the Moraines of the Georgian Bay Lobe of the Ice-Sheet." By FRANK B. TAYLOR.

"Notes on the Geology of Montreal and Vicinity." By F. D. ADAMS.

"Notes on the Geology of the Rocky Mountains of Montana." By WALTER H. WEED.

"Marine Cretaceous Formations in Deep Wells in Southeastern Virginia." By N. H. DARTON.

"The Cretaceous Series of the West Coast of Greenland." By CHARLES SCHUCHERT and DAVID WHITE.

"Note on Lepidophloios Cliftonensis." By SIR WILLIAM DAWSON.

"Omphalophloios, a new Lepidodendroid type." By DAVID WHITE.

"The Mastodon in Western Ontario." By H. M. AMI.

"Mastodon and Mammoth Remains Found near Hudson Bay." By ROBERT BELL.

"Fossil-like Forms in Sault Ste. Marie Sandstone." By ROBERT BELL.

"Weathering of Alnoite in Manheim, N. Y." By C. H. SMYTH, JR.

"Syenite-Porphyry Dikes in the Adirondack Region. By HENRY P. CUSHING.

"Nodular Granite from Pine Lake, Ontario." By FRANK D. ADAMS.

"Chemical Composition of the Granite from Pine Lake, Ontario." By NEVIL N. EVANS.

"Clastic Huronian Rocks of Western Ontario, and the Relation of Huronian to Laurentian." By A. P. COLEMAN.

"The Classification of Igneous Rocks." By JOSEPH P. IDDINGS.

"The Geological *versus* the Petrographical Classification of Igneous Rocks." By WHITMAN CROSS.

"Geological Probabilities as to Petroleum." (President's address.) By EDWARD ORTON.

"On the Occurrence of Corundum in North Hastings, Ont." By A. E. BARLOW.

"The Harvard Geographical Models." W. M. DAVIS.

"Experiments on the Flow of Rocks, now being conducted at McGill University." By FRANK D. ADAMS and JOHN T. NICOLSON.

"Estimates and Causes of the Shortening of the Outer Part of the Earth." By C. R. VAN HISE. (By title.)

"New Geothermal Data from South Dakota." By N. H. DARTON.

"Concentric Weathering in Sedimentary Rocks." By T. C. HOPKINS.

"Note on an Area of Compressed Structure in Western Indiana." By GEORGE H. ASHLEY.

T. C. C.

Authors' Abstracts.

PAPERS READ AT THE MONTREAL MEETING OF THE GEOLOGICAL SOCIETY OF AMERICA.

Sands and Clays of the Ottawa Basin. By R. W. Ells.

The paper describes the leading physical features of the Ottawa River basin, which comprises about 130,000 square miles, and which includes numerous large streams, both from the north and south. The elevations of the height of land on the north, which divides the Ottawa waters from those which flow into James Bay, are given as ranging from 900 feet at Grand Lake, one of the large expansions in the upper part of the Ottawa River, to about 1100 feet near the source of the stream. Further east at the head of the St. Maurice, the elevation is somewhat greater, probably between 1300 and 1400 feet. The height of the divide to the north of Lake Temiscaming is given as 923 feet, while the elevation of Lake Temagami, which empties from the south into Lake Huron and from the northeast into the Ottawa, is 967 feet above sea. The height of land along the southern rim of the basin ranges from 645 feet at Lake Nipissing to about 1400 feet at the head waters of the Petewawa and Madawaska rivers, decreasing to 417 feet at the head of the Rideau.

Over all this area to the height of land, apparently continuous deposits of a blue clay, similar in character to the recognized marine clays of the lower Ottawa and St. Lawrence basins, can be seen. These have in places a thickness of over 100 feet, and they are overlaid over a great portion of the basin by sands, similar in character to the well-known Saxicava sands which contain marine fossils further east. In the eastern part of this area well defined deposits of these clays and sands holding marine shells, are found at elevations of nearly or quite 600 feet above sea level, while shore lines and old beaches, also of marine origin, are to be seen along the north side of the St. Lawrence, as well as along the lower Ottawa, which range in elevation from 600 to at least 1000 feet above present sea level. The bones of a whale

have been obtained from a large kame-like ridge near Smith's Falls at a height above sea of 440 feet, the skeleton being reached in the excavation at a point 50 feet from the outer zone of the ridge. This elevation is nearly 200 feet above the present level of Lake Ontario. The indications of submergence, as seen along the sides of the Montreal mountain, are clear at elevations from 220 to 750 feet or nearly to the summit. Similar high level beaches are found along the slopes of the mountain ranges in eastern Quebec to a height of nearly, or in places quite 1000 feet, so that it may be considered as fairly well established by the latest evidence that this portion of Canada was submerged, subsequent to the glacial period, to a depth of at least 1000 to 1200 feet. All the high level beaches in the St. Lawrence and Ottawa valleys front directly to the open estuary of the St. Lawrence River, and there were, in so far as can now be seen, no barriers to interrupt the inland spread of the sea between that river and the upper great lakes. The submergence mentioned would carry these waters over the height of land in nearly every part of the Ottawa basin and support the view now put forward that the clays which are found on most of the portages at the highest levels are marine. It is probable also on this theory that the Erie clays, which are very similar in character to those of the lower Ottawa, and unconformable to the overlying series, are the true equivalents of the Leda clays of the eastern area, though this point requires further detailed examination before the exact relations of the fresh water deposits of this part of Canada to the underlying clays can be completely established.

Note on an Area of Compressed Structure in Western Indiana. By GEO. H. ASHLEY.

As is well-known, the rocks of Illinois, most of Indiana, and part of Kentucky form a basin with gently dipping slopes, and where local evidences of stress are observed they usually indicate tension. Thus the faults are commonly tension or normal faults, and the joints are perpendicular and open. Recently several local areas of disturbance have been observed and this note is intended to call attention to one of these. In this case the evidence of considerable tangential pressure is seen in (1) overthrust or pressure faults, (2) coal beds compressed laterally until they become several times their original thickness; (3) in place of perpendicular open joints, confined to the coal

and with faces even but not polished or showing any indication of rubbing, such as are common in the region as a whole, are found regular oblique joints, cutting the roof and floor as well as the coal, and having their faces much slickened. These joints are evidently shearing planes, and show signs of incipient movement, or in some cases, as in one of the figures given, they become fault planes allowing the lower of two coal beds twenty feet apart to slide an unknown distance up over the upper bed. The main system of joints dip to the north, a second set sometimes developed dip to the south. The pressure acted with the general strike of the rocks. The area described covers a few square miles southwest of Asherville, Clay county, Ind. The structure has been disclosed by the operations of mining.

Syenite-porphyry Dikes in the Adirondacks. By H. P. CUSHING.

Interesting dikes of pre-Potsdam age have been recently found in the Adirondacks which constitute the complementary rocks to the diabases of the region. Basal conglomerates of the Potsdam contain pebbles derived from them. On the other hand, together with the diabases, they comprise the only unmetamorphosed pre-Potsdam rocks known in the district.

These dike rocks are made up of feldspar, quartz, and biotite, with or without hornblende, and with accessory iron ores, apatite, augite, titanite. Both orthoclase and albite feldspar are present, commonly intergrown as microperthite. The rock is porphyritic, both feldspar and biotite occurring in two generations, the latter only sparingly. The feldspar largely predominates over all other minerals, constituting from 60 to 80 per cent. of the rock. The ground mass structure is trachytic or orthophyric.

Chemically the rocks are characterized by high alkali and low lime-magnesia percentage, and rather low ratio of alumina to silica. They belong to the alkali-syenite group of Rosenbusch. Their bunched distribution and mineralogic similarity indicate that they were products of a common magma, and if that be so they afford an interesting case of magmatic differentiation as they range from 69 to 52 per cent. of silica. With decreasing silica the lime, magnesia and alkali percentages rise, the latter retain their preponderance.

The greater number of the dike rocks would be classed as syenite-porphyries, nordmarkite, and pulaskite types. The more acidic repre-

sentatives belong properly with the alkali-granites. The most basic rocks represent an undescribed type, so far as the writer is aware, being very basic mica-syenite-porphyries.

Together with the diabases these rocks form an eruptive assemblage quite similar to that which characterizes Keweenawan time in the upper lake region, nor, since there is in each case the same relationship to a younger unconformable sandstone of upper Cambrian age, and to an older mass of gabbroic intrusives, can they depart widely from them in age.

Leaving out of the question the older gneisses of somewhat doubtful origin, there were three distinct periods of igneous activity in the Adirondack region. The earliest gave rise to gabbros and granites, the next to diabases and syenite-porphyries, the last to bostonites and various basic rocks (camptonites, monchiquites, etc.). Analyses show an astonishing agreement between the acidic rocks of each period on the one hand and the basic rocks on the other. Though a long time interval elapsed between each, magmatic relationship seems not unlikely.

Accumulating evidence seems to the writer to indicate the possibility that three similar periods were characteristic of the entire shore line of the Canadian and Appalachian protaxes, and that such possibility should be added to the working hypotheses of all workers in that field.

(Other Abstracts, Reviews, and Publications deferred to next number.)